ESSAYS

And other

TRACTS

Written at distant Times, and on several [illegible]

By the Honourable

ROBERT BOYLE.

The Second Edition,

Wherein some of the Tracts are enlarged by Experiments, and the Work is increased by the Addition of a Discourse about the

ABSOLUTE REST

in

BODIES.

LONDON,

Printed for *Henry Herringman* at the *Blew Anchor* in the Lower Walk of the *New Exchange*, MDCLXIX.

Presented to
The University Library
Cambridge
by the Representatives of the
Rev. JOSEPH GRIFFITH, LL.D.
St John's College
1895.

09... 1214

PHYSIOLOGICAL

ESSAYS. Vrz

page. Concerning experimentall Essays
3 The unsuccessfulnes of Experim^ts
6 And unsuccessfulnes of them
9 page Specimens to make Chimicall Experiments
61 — History of Fluidity & Firmenes
Of Absolute Rest

LONDON,
Printed for *Henry Herringman*, MDCLXIX.

PHYSIOLOGICAL

ESSAYS.

AN *ADVERTISEMENT* TO THE READER:

Prefixed to the First Edition,
Put forth *A. D.* 1661.

THE *Publiſher deſires that the Reader ſhould be inform'd, that neither were the following Treatiſes written near about the ſame Time (ſome of them being divers years elder than ſome others) nor yet are they now publiſhed in the ſame Order that they were written in. For the firſt of theſe Diſcourſes (though penned about four years ſince) was not only written after the ſecond, third, and fourth, but after divers other Eſſays which the Author has yet lying by him among his Papers; it being intended for a kind of Introduction to all thoſe Treatiſes which under ſeveral names, but chiefly that of* Phyſiological Eſſays, *the Author had then compos'd. But having during the late Confuſions ſo diſpoſed of his Papers to ſecure*

them, that he could not himſelf ſeaſonably recover them; and being engaged by Promiſe to ſome friends, to let about half a dozen of his ſmall Tracts come abroad into the world by ſuch a time, he was fain to ſend the following Treatiſes to the Preſs as they came, ſome at one time, ſome at another, to his hands. And this his occaſions did now and then reduce him to do in ſuch haſte, that he could not attend the correcting either the Printers Lapſes or his own, and particularly was obliged, partly by haſte, and partly by a Diſtemper in his Eyes, to ſend away the Hiſtory of Firmneſs *without ſo much as reading it over. All which 'tis hoped the Equitable Reader will conſider in his favour, and bear with what may be imputable to ſuch Circumſtances.*

I ſhould add nothing further, were it not that to ſave the Reader the trouble of gueſſing who is meant by that Pyrophilus *to whom moſt of the following Treatiſes are addreſſed, I think it requiſite to inform him, that the perſon veil'd under that name, is that hopeful Toung Gentleman Mr.* Richard Jones, *only Son to the Lord Viſcount* Ranelagh *and an Excellent Lady, Siſter to the Author.*

AN

AN ADVERTISEMENT About the Second Edition.

For the eaſe of thoſe that had already peruſed the firſt Edition of the Phyſiological Eſſays, *and other Tracts that accompany'd them, the Author took the pains to include the principal things added in the preſent Edition in* Paratheſes, *that the chief Additions may be found out without the pains of reading over again the whole Book. But finding theſe* Paratheſes *to have been, by an overſight of the Preſs, omitted, ſome amends was thought fit to be endeavoured to be made for that, by intimating that the Experiments that enlarge this New Edition, are, for the moſt part, to be found in the following Pages of it*

Page 196. 202. 222. 226. 227. 241. 243. 246. from pag. 252 to 255. p. 257. p. 238, 239. p. 263. from p. 289 to 291. p. 32 and 33. p. 169, 170.

A PROEMIAL ESSAY,

WHEREIN,

With ſome conſiderations touching

EXPERIMENTAL ESSAYS

in General,

Is interwoven ſuch an Introduction to all thoſe written by the Author, as is neceſſary to be perus'd for the better underſtanding of them.

I Know not, *Pyrophilus*, whether what you will meet with in the enſuing Diſcourſes will prove worthy of your taking notice of it: Yet I dare tell you, that if all my Endeavours to ſerve you were not Duties, I ſhould think I might deſerve your Thanks for venturing to write them for your ſake. For I am ſufficiently ſenſible both how unlearned I am, and in how learned an Age I preſume to write: Nor has the great number of thoſe eſcap'd my Obſervation, who finding it a much eaſier task to cenſure Experimental Compoſures than to write ſuch, endeavour to acquire the Title of Judicious, by condemning all things that themſelves have not written, or thought on. And indeed, *Pyrophilus*, I had beſides theſe, ſo many other diſcouraging Conſiderations in my Eye, whilſt I was ſetting down the following Eſſays, that I ſhould ſcarce have proſecuted a Deſign ſo full of trouble, and ſo unlikely by its ſucceſs to make amends for

it, if I had thought it free for the ſecuring of my own Quiet and Credit, to ſuppreſs Obſervations which might prove ſerviceable to you, who having ſufficiently convers'd with Books, are now deſirous to begin to converſe with Things themſelves. But I muſt confeſs, I look upon Experimental truths as Matters of ſo great concernment to Mankind, that in ſpight of the juſt ſenſe I have of my own Diſabilities, I am deterr'd from complying with thoſe Inclinations and Motives that endear ſilence to me, by conſidering the Fate of him, who though he had leſs entruſted to him than any of his Companions, was yet ſeverely puniſht for burying his ſingle Talent. And though, *Pyrophilus*, I could not without ſuch reluctancy reſolve to write, yet I found it much more uneaſie to reſolve to write ſo ſoon: For I could not but conſider, that being yet but very young, not only in Years, but, what is much worſe, in Experience, I have yet much more need to learn, than ability to teach; and I conſider'd too, that after a man is grown ſomewhat acquainted with things themſelves, and has taken ſome general notice of the Cognations, Differences, and Tendencies of their Properties, he may every day ſo much improve his Knowledge, that I am apt to think, that if God ſhould be pleas'd to protract my Life a few years longer, I ſhall at the end of them be able to look upon what I have hitherto written with Pity, if not with Bluſhes. And I have often obſerv'd, that it is wont to happen in the productions of the Mind, as in thoſe of the Body. For as thoſe that apply themſelves to Procreation too young, and before they have attain'd to their full vigour and ſtrength, do generally both hinder their own growth, and become the Parents but of weak and ſhort-liv'd Children; ſo they that too early, and before their Judgment and Experience be fully ripe, addict themſelves to write Books, do commonly both hinder their own Proficiency in Knowledge, and write but immature,

ture, and therefore ſeldom laſting Treatiſes. Nor ſhould I, *Pyrophilus*, have ever prevail'd with my ſelf to preſent you ſo early theſe Diſcourſes, ſince by keeping them longer by me, I might eaſily by ſecond Thoughts, and freſh Experiences be enabled to correct and enrich them, did not the frequent and dangerous diſtempers to which my very ſickly Conſtitution has of late render'd me obnoxious, make me juſtly doubt, whether or no, if I ſhould long forbear to write, Death would not ſooner come than the expected Maturity of Age and Judgment. And though I had no ſuch Conſideration to move me to make haſt to tender to you the enſuing Diſcourſes, yet this would ſuffice to engage me to preſent them you with all their preſent defects; that if I ſhould keep them till I can make them leſs unworthy of you, I muſt keep them till you are grown paſt the need of them.

And now that I have told you, *Pyrophilus*, both why I have written the enſuing Diſcourſes, and why I keep them not by me long enough to preſent them you with fewer Imperfections, I ſuppoſe you will expect that I ſhould next tell you why I have caſt them into Eſſays, rather than into any other form. To ſatisfie you about this particular, *Pyrophilus*, I muſt freely acknowledge to you, that it has long ſeem'd to me none of the leaſt impediments of the real advancement of true Natural Philoſophy, that men have been ſo forward to write Syſtems of it, and have thought themſelves oblig'd either to be altogether ſilent, or not to write leſs than an entire body of Phyſiology: for from hence ſeem to have enſu'd not a few Inconveniences.

And firſt, when men by having diligently ſtudy'd either Chymiſtry, Anatomy, Botanicks, or ſome other particular part of Phyſiology, or perhaps by having only read Authors on thoſe Subjects, have thought themſelves thereby qualify'd to publiſh compleat Syſtems of Natural Philo-

ſophy, they have found themſelves by the nature of their undertaking, and the Laws of Method, engag'd to write of ſeveral other things than thoſe wherein they had made themſelves Proficients, and thereby have been reduc'd, either idly to repeat what has been already, though perhaps but impertinently enough, written by others on the ſame Subjects, or elſe to ſay any thing on them rather than nothing, leſt they ſhould appear not to have ſaid ſomething to every part of the Theme which they had taken upon themſelves to write of.

In the next place, the ſpecious and promiſing Titles and comprehenſive Method of theſe Syſtems have been often found to perſwade unwary Readers, that all the parts of Natural Philoſophy have been already ſufficiently explicated, and that conſequently it were needleſs for them to put themſelves to trouble and charges in making further Enquiries into Nature, ſince others having already ſufficiently made it their buſineſs to inveſtigate and explicate Phyſiological Truths, our buſineſs needs now be no more than to learn what they have taught, and thankfully to acquieſce in it.

Nor has the Syſtematical way of writing been prejudicial only to the proficiency of ſome Readers, but alſo to the reputation of ſome Writers of Syſtematical Books. For it not unfrequently happens, that when a Writer, to vent ſome few peculiar Notions or Diſcoveries of his own, preſumes to write a whole body of Philoſophy, what is truly his own, though excellent in its kind, is either loſt in the Crowd of the things he has borrow'd from others, and ſo comes to be over-look'd, or at leaſt not ſufficiently taken notice of, by the Reader; or elſe the unwelcome, and yet in ſuch Compoſures ſcarce evitable, Repetition of many things that others had I know not how often written before, occaſions the laying aſide of the whole Book, as a Rhapſody of trite and vulgar Notions, ſcarce worth the peruſing:

perusing: and by this means the Author often loses the Reputation of his peculiar Notions, as well as the Reader the benefit of them; and that which would have made an excellent and substantial Essay, passes but for a dull and empty Book.

But the worst Inconvenience of all is yet to be mention'd, and that is, That whilst this Vanity of thinking men oblig'd to write either Systems or Nothing, is in request, many excellent Notions or Experiments are by sober and modest men suppress'd, because such Persons being forbidden by their Judgment and Integrity to teach more than they understand, or assert more than they can prove, are likewise forbidden by Custome to publish their Thoughts and Observations, unless they were numerous enough to swell into a System. And indeed it may be doubted whether the Systematical Writers have not kept the world from much more useful Composures than they have presented it with. For there are very few men, if any at all, in the world, that are enrich'd with a competent stock of Experiments and Observations to make out clearly and solidly, I say not all the Phænomena of Nature, but all those that belong to Chymistry, Anatomy, or any such considerable subordinate Doctrine of Physiology. And those very men that are diligent and judicious enough to study prosperously any of those parts of Physiology, are oblig'd to spend so much time in the accurate Prosecution of that, and are wont to be thereby made so wary, and so thorowly acquainted with the difficulty of Physiological Investigations, that they will least of all men be forward to write Systems.

And what I say, *Pyrophilus*, of the inconveniences that have hitherto been observ'd to flow from mens forwardness to write entire bodies of Philosophy, may be in its degree appli'd to the practice of those that pretend to give us compleat accounts of Chymistry, or almost (I say almost) any

any other conſiderable and comprehenſive part of Natural Philoſophy: Though I deny not, that in ſuch attempts which are much leſs difficult than the former, ſome Men have done Mankind conſiderable ſervice, though they have not fully perform'd what the Titles of their Writings ſeem to promiſe. Nor am I ſo rigid as to be unwilling that from time to time ſome very knowing Writer ſhould publiſh a Syſtem of Phyſiology, or any part of it, according to the beſt Authors and Obſervations of that time: For ſuch a Work may be uſeful, partly, for the inſtructing of Youth in Schools and Academies; and partly, that men may have from time to time an Inventory of what hath been already diſcover'd, whereby the needleſs labour of ſeeking after known things may be prevented, and the progreſs of Mankind as to Knowledge might the better appear. But then it is to be wiſh'd that ſuch Writings were not publiſh'd but by very intelligent Perſons, nor till ſome conſiderable improvement have been made in Knowledge ſince the laſt work of that Nature. Nor would I be thought to diſallow ſuch Writings of very Learned Men, as though they may bear very general Titles, yet are not publiſh'd by their Authors as compleat Bodies or Syſtems of Phyſiology, but rather as general Principles (almoſt like the Hypotheſes of Aſtronomers) to aſſiſt men to explicate the already-known Phænomena of Nature. For of ſuch kind of Writings, if their Authors be (as for the moſt part they are) ſubtle and inquiſitive men, there may be very good uſe, not ſo much by their gratifying the Intellect with the plauſible account of ſome of Natures Myſteries; as becauſe on the one ſide their Writers, to make good their new Opinions, muſt either bring New Experiments and Obſervations, or elſe muſt conſider thoſe that are know nalready after a new Manner, and thereby make us take notice of ſomething in them unheeded before; and on the other ſide, the curioſity of Readers, whether they like

like or disapprove the Hypothesis propos'd, is wont to be thereby excited to make trial of several things, which seeming to be Consequences of this new Doctrine, may by their proving agreeable or repugnant to Experiment either establish or overthrow it.

And that you may know, *Pyrophilus*, what kind of Writings I mean, I shall name to you the Learned *Gassendus* his little *Syntagma* of *Epicurus*'s Philosophy, and that most ingenious Gentleman *Mons*r *Des-Cartes* his Principles of Philosophy. For though I purposely refrain'd, though not altogether from transiently consulting about a few Particulars, yet from seriously and orderly reading over those excellent (though disagreeing) Books, or so much as Sir *Francis Bacon's Novum Organum*, that I might not be prepossess'd with any Theory or Principles till I had spent some time in trying what Things themselves would incline me to think; yet beginning now to allow my self to read those excellent Books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enrich'd the ensuing Essays with divers truths which they now want, and have explicated divers things much better then I fear I have done. But of such Writers the number is yet (and will I fear always be) so small, that I shall not need to make many Exceptions when I treat of the usefulness of writing Books of Essays, in comparison of that of writing Systematically: Or at least, *Pyrophilus*, whilst I presume not to judge of other mens abilities, I hope it may be lawful for me to confess freely to you concerning my self, that I am very sensible of my being far from having such a stock of Experiments and Observations, as I judge requisite to write Systematically; and I am apt to impute many of the Deficiencies to be met with in the Theories and Reasonings of such great Wits as *Aristotle*, *Campanella*, and some other celebrated Philosophers, chiefly to this very thing, that they have too hastily,

haſtily, and either upon a few Obſervations, or at leaſt without a competent number of Experiments, preſum'd to eſtabliſh Principles, and deliver Axioms. For it very rarely otherwiſe happens, than that Theories that are grounded but upon few and obvious Experiments are ſubject to be contradicted by ſome ſuch Inſtances as more free and diligent Enquiries into what of Nature is more abſtruſe, or even into the leſs obvious Qualities of things, are wont to bring to light. I remember, that being once at *Leyden*, I was brought to the Top of a Tower, where in a darken'd room (ſuch as is now uſed in many places to bring in the Species of external Objects) a Convex glaſs, apply'd to the only hole by which light was permitted to enter, did project upon a large white ſheet of Paper, held at a juſt diſtance from it, a lively repreſentation of divers of the chief Buildings in the Town, all which upon the admiſſion of more light into the room, by opening the Window, did immediately diſappear. And methinks, *Pyrophilus*, that in divers of the Philoſophical Theories that have been formerly applauded, ſomething not unlike this may be eaſily obſerv'd: for though, whilſt they are look'd on with ſuch a weak and determinate degree of light, they may appear very artificial and well-proportion'd Fabricks, yet they appear ſo but in that twilight, as it were, which is requiſite to their conſpicuouſneſs. For if but a full light of new Experiments and Obſervations be freely let in upon them, the Beauty of thoſe (delightful, but Phantaſtical) ſtructures does immediately vaniſh.

And truly, *Pyrophilus*, if men could be perſwaded to mind more the Advancement of Natural Philoſophy than that of their own Reputations, 'twere not methinks very uneaſie to make them ſenſible, that one of the conſiderableſt ſervices that they could do Mankind, were to ſet themſelves diligently and induſtriouſly to make Experiments and collect Obſervations, without being over-forward

ward to eſtabliſh Principles and Axioms, believing it uneaſie to erect ſuch Theories as are capable to explicate all the Phænomena of Nature, before they have been able to take notice of the tenth part of thoſe Phænomena that are to be explicated. Not that I at all diſallow the uſe of Reaſoning upon Experiments, or the endeavouring to diſcern as early as we can the Confederations, and Differences, and Tendencies of things: For ſuch an abſolute ſuſpenſion of the exerciſe of Reaſoning were exceeding troubleſome, if not impoſſible. And as in that Rule of Arithmetick which is commonly called *Regula falſi*, by proceeding upon a conjecturally-ſuppoſed Number, as if it were that which we enquire after, we are wont to come to the knowledge of the true number ſought for: ſo in Phyſiology it is ſomtimes conducive to the diſcovery of truth, to permit the Underſtanding to make an Hypotheſis in order to the Explication of this or that difficulty, that by examining how far the Phænomena are, or are not, capable of being ſalv'd by that Hypotheſis, the Underſtanding may even by its own Errors be inſtructed. For it has been truly obſerv'd by a great Philoſopher, That Truth does more eaſily emerge out of Error than Confuſion. That then that I wiſh for, as to Syſtems, is this, That men in the firſt place would forbear to eſtabliſh any Theory, till they have conſulted with (though not a fully competent number of Experiments, ſuch as may afford them all the Phænomena to be explicated by that Theory, yet) a conſiderable number of Experiments in proportion to the comprehenſiveneſs of the Theory to be erected on them. And in the next place, I would have ſuch kind of ſuperſtructures look'd upon only as temporary ones, which though they may be preferr'd before any others, as being the leaſt imperfect, or, if you pleaſe, the beſt in their kind that we yet have, yet are they not to entirely be acquieſced in, as abſolutely perfect, or uncapable of improving Alterations.

It were very poſſible, *Pyrophilus*, to let you ſee that all that has been ſaid to recommend to you that form of Writing which (in imitation of the French) we call Eſſayes, is but a part of what may be pertinently ſaid to the ſame purpoſe. But becauſe this Introductory Diſcourſe it ſelf is to be but an Eſſay, not a Book, I dare not long inſiſt upon the Advantages of this ſort of Diſcourſes. Only becauſe I think that if I could engage you, *Pyrophilus*, and ſuch other ingenious Perſons, to caſt their Phyſiological Obſervations and Reflexions into Experimental Eſſayes, I ſhould thereby do real Learning no trifling ſervice, by bringing ſo uſeful a way of writing into the requeſt it deſerves; Upon this conſideration, I ſay, I muſt beg leave to repreſent to you this great Conveniency of Eſſayes, That as in them the Reader needs not be clogg'd with tedious Repetitions of what others have ſaid already, ſo the Writer, having for the moſt part the Liberty to leave off when he pleaſes, is not oblig'd to take upon him to teach others what himſelf does not underſtand, nor to write of any thing but of what he thinks he can write well. And if ſuch Eſſayes be but as they ſhould be competently ſtock'd with Experiments, 'tis the Readers own fault if he be not a Learner by them: for indeed when a Writer acquaints me only with his own Thoughts or Conjectures, without enriching his diſcourſes with any real Experiment or Obſervation, if he be miſtaken in his Ratiocination, I am in ſome dang r of erring with him, and at leaſt am like to loſe my time, without receiving any valuable Compenſation for that great loſs: but if a Writer endeavours, by delivering new and real Obſervations or Experiments, to credit his Opinions, the Caſe is much otherwiſe; for let his Opinions be never ſo falſe, his Experiments being true, I am not oblig'd to believe the former, and am left at liberty to benefit my ſelf by the later; and though we have erroneouſly ſuperſtructed upon his Experiments, yet the foun-

foundation being ſolid, a more wary builder may be very much further'd by it in the erection of more judicious and conſiſtent Fabricks: ſuch a Writer, if I be not wanting to my ſelf, will certainly teach me uſeful Truths, and if it be not my fault, he can lead me into no errors; and oftentimes the very Experiments that he delivers, beſides that they may be applicable to many other purpoſes unthought of by him, may be either ſufficient or at leaſt helpful to the very diſcovery of the erroneouſneſs of the Opinions they are alledg'd to countenance: and I make account that a man that gives me, together with his conjectures (though erroneous) in matters of Phyſiology, ſome noble Experiment or Obſervation by which he pretends to verifie them, does me no greater injury than *Galileo* upon his firſt Invention of the Teleſcope would have done an Aſtronomer, if he had told him, that he had diſcover'd in Heaven thoſe imaginary new Stars which a late Mathematician has fancy'd himſelf to have deſcry'd there, and at the ſame time had made him a Preſent of an excellent Teleſcope, with expectation that thereby the Receiver ſhould be made of the Giver's Opinion; for by the help of his Inſtrument the Aſtronomer might not only make divers uſeful Obſervations in the Sky, and perhaps detect new Lights there, but diſcern alſo his miſtake that gave it him.

After what has been ſaid, *Pyrophilus*, of the Uſefulneſs of experimental Eſſays, we muſt proceed to ſay ſomething concerning the Manner of writing them: but becauſe you will alſo expect to receive ſome account of the enſuing Diſcourſes, I ſhall not treat of thoſe two Subjects apart, but, in diſcourſing of the following Eſſays, ſhall take occaſion to acquaint you with part of my thoughts concerning ſuch kind of Compoſures in general, the other Conſiderations belonging to the ſame Subject being already upon ſeveral Occaſions diſpers'd among, and to be met with in, the enſuing Diſcourſes themſelves.

And firſt, as for the ſtyle of our experimental Eſſays, I ſuppoſe you will readily find that I have endeavour'd to write rather in a Philoſophical than a Rhetorical ſtrain, as deſiring that my expreſſions ſhould be rather clear and ſignificant, than curiouſly adorn'd: For, to a ſubject of the ſerious and important Nature of Phyſiology, that ſaying may unqueſtionably be appli'd, *Ornari res ipſa negat, contenta doceri.* And certainly in theſe Diſcourſes, where our deſign is only to inform Readers, not to delight or perſwade them, Perſpicuity ought to be eſteem'd at leaſt one of the beſt Qualifications of a ſtyle, and to affect needleſs Rhetorical Ornaments in ſetting down an Experiment, or explicating ſomething abſtruſe in Nature, were little leſs improper than it were (for him that deſigns not to look directly upon the Sun it ſelf) to paint the Eye-glaſſes of a Teleſcope, whoſe clearneſs is their Commendation, and in which even the moſt delightful Colours cannot ſo much pleaſe the eye as they would hinder the ſight. And that it may not be ſuſpected, that thoſe that would not have it requiſite to imploy a florid ſtyle in treating of Philoſophical Subjects, do but in their own excuſe deny the neceſſity of ſuch Rhetorical Embelliſhments as they are not able to afford their Compoſures, give me leave to ſubjoyn, that it was not an unpoliſh'd Naturaliſt, but that Prince of Orators, *Cicero* himſelf, who made this ſtudious Declaration, *Omne* (ſays he) *quod de re bona dilucidè dicitur, præclarè mihi dici videtur: iſtiuſmodi autem res velle ornatè dicere, puerile eſt; planè autem & perſpicuè expedire poſſe, docti & intelligentis Viri.* But I muſt not ſuffer my ſelf to ſlip unawares into the Common place of the unfitneſs of too ſpruce a ſtyle for ſerious and weighty matters; and yet I approve not that dull and inſipid way of writing which is practis'd by many Chymiſts, even when they digreſs from Phyſiological Subjects: for though a Philoſopher need not be ſollicitous that his ſtyle

ſtyle ſhould delight its Reader with his Floridneſſe, yet I think he may very well be allow'd to take a Care that it diſguſt not his Reader by its Flatneſs, eſpecially when he does not ſo much deliver Experiments or explicate them, as make Reflections or Diſcourſes on them; for on ſuch Occaſions he may be allow'd the liberty of recreating his Reader and himſelf, and manifeſting that he declin'd the Ornaments of Language, not out of Neceſſity, but Diſcretion, which forbids them to be us'd where they may darken as well as adorn the Subject they are appli'd to. Thus (to reſume our former Compariſon) though it were fooliſh to colour or enamel upon the glaſſes of Teleſcopes, yet to gild or otherwiſe embelliſh the Tubes of them, may render them more acceptable to the Uſers, without at all leſſening the Clearneſs of the Object to be look'd at through them.

And as for Exotick Words and Terms borowed from other Languages, though I expect that Perſons not converſant in the Philoſophical Compoſures written (eſpecially of late) in our Language will be apt to ſuſpect me for the willing Author of divers new Words and Expreſſions, yet as for you, *Pyrophilus*, who peruſe other then Moral, Theological, and Hiſtorical Books in Engliſh, and find how much uſe is made in them of Exotick Terms, I hope you will find that I have not at all affected them, but have rather ſtudiouſly declin'd the uſe of thoſe which Cuſtom has not render'd familiar, unleſſe it be to avoid the frequent and unwelcome Repetition of the ſame word, (ſo troubleſome to the Ear, and ſo much forbidden by Orators) or for ſome peculiar ſignificancy of ſome ſuch Word, whoſe Energy cannot be well expreſs'd in our Language, at leaſt without a tedious Circumlocutioo. And in ſuch caſes, *Pyrophilus*, I ſuppoſe a Writer may be allow'd to uſe Exotick Terms, eſpecially when Cuſtom has not only Denizon'd them, but brought them into requeſt. For as in the Faſhions of Clothes, though perhaps Fools begin them

them, yet Wiſe men, when they are once generally receiv'd, ſcruple not to follow them, becauſe then obſtinately to decline them would be as ridiculouſly ſingular as at firſt it would have been to begin them: ſo in Exotick Words, when Cuſtom has once made them familiar and eſteem'd, ſcrupulouſly to decline the uſe of them may be as well a fault, as needleſly to imploy them: For it is not the Uſe but the Affectation of them that is unworthy a Philoſopher. And from the latter of thoſe I hope I have kept my ſelf far enough: For I ſhould think my ſelf guilty of a very Childiſh vanity, if the uſe I made of Languages were ſo to write as to be the leſs underſtood. But beſides the unintentional deficiencies of my ſtyle, I have knowingly and purpoſely tranſgreſs'd the Laws of Oratory in one particular, namely, in making ſometimes my Periods or Parentheſes over-long: for when I could not within the Compaſs of a regular Period compriſe what I thought requiſite to be delivered at once, I choſe rather to neglect the Precepts of Rhetoricians, than the mention of thoſe things which I thought pertinent to my Subject, & uſeful to you, my Reader. And for this fault, *Pyrophilus*, ſince I have made my ſelf guilty of it but for your ſake, I think I ought to obtain your pardon at leaſt as eaſily as my own, ſince barely to keep you from loſing any thing that I conceiv'd might be ſerviceable to you, I knowingly expoſe my ſtyle to be cenſur'd as diſproportionate to it ſelf.

The next thing, *Pyrophilus*, of which I am to give you an account, is, why I have in the enſuing Eſſays deliver'd many Experiments and Obſervations, which may ſeem ſlight and eaſie, and ſome of them obvious alſo, or elſe perhaps mention'd by others already. To ſatisfie you about this, I muſt inform you that many of the Particulars which we are now conſidering, were in my firſt Deſign collected in order to a Continuation of the Lord *Verulam's Sylva Sylvarum*, or Natural Hiſtory. And that my intended Centuries

Centuries might reſemble his, to which they were to be annex'd, it was exquiſite that ſuch kind of Experiments and Obſervations as we have been newly ſpeaking of, ſhould make up a conſiderable part of them. And indeed it were to be wiſh'd, that ſuch inquiſitive Perſons as cannot be at the Charge, or have not the opportunity, of making new Experiments, would buſie themſelves, as they have opportunity, in induſtriouſly collecting and carefully ſetting down the Phænomena to be met with without the Aſſiſtance of new Experiments, eſpecially ſuch particulars as ſeem either to be of moment in order to the hinting or Confirmation of ſome Conſiderable Truth, or to the Detection of ſome Applauded Error, or elſe to have bin, though obvious enough, yet little taken notice of. For I am confident that very much may be done towards the Improvement of Phyſiology by a due Conſideration of and reflexion on the obvious Phænomena of Nature, and thoſe things which are almoſt in every bodies power to know, if he pleaſe but ſeriouſly to heed them; and I make account that attention alone might quickly furniſh us with one half of the Hiſtory of Nature, as well as induſtry is requiſite, by new Experiments, to enrich us with the other. And therefore I confeſs I think my ſelf beholding to him that firſt makes me take notice of what I might eaſily have known, but heeded not before; it not ſeldom happening, that we are prejudic'd by, though we do not complain of that ignorance from which we might relieve our ſelves, if we did but diligently turn our eyes to the Obſervations wherewith even neighbouring and familiar Objects would, if duly conſulted, preſent us. But I digreſs, and therefore muſt ſtep back into the way, and tell you, that the reaſons why I firſt deſign'd the Narrative of what I had try'd and obſerv'd for a Continuation of Sir *Francis Bacons* Natural Hiſtory, you will meet with in my Preface to that ſpecimen of the intended Continuation, which I have given in thoſe

of

of my Eſſays that treat Of Promiſcuous Experiments: and the reaſon why I have ſince declin'd that ſuccinct way of Writing, is, for the ſake of *Pyrophilus*, that I might have, in a more free and uncircumſcribed way of diſcourſing, a greater Liberty to inſiſt on and manifeſt the Reaſonableneſs of ſuch Animadverſions as I thought ſeaſonable for a Perſon, who, though a great Proficient in the other parts of Philoſophy, is but a Beginner in Experimental Learning. And the ſecond Reaſon why I have often made uſe of ſeemingly ſlight Experiments, is, becauſe ſuch are more eaſily and cheaply try'd, and they being alledg'd for the moſt part to prove ſome Aſſertion, or credit ſome Admonition, I thought their Eaſineſs or Obviouſneſs fitter to recommend them, than depretiate them; and I judg'd it ſomewhat unkind, or at leaſt indiſcreet, to refer you moſt commonly for proof of what I deliver'd, to ſuch tedious, ſuch difficult, or ſuch intricate Proceſſes, as either You can ſcarce well make, unleſs You be already what I deſire my Experiments ſhould help to make You, a skilful Chymiſt; or elſe are as difficult to be well judg'd, as the truth they ſhould diſcover is to be diſcern'd. I was alſo hopeful that the Eaſineſs of divers things inviting you to make tryal of them, and keeping You from being diſappointed in Your Expectations, the ſucceſs of Your firſt attempts would incourage You to make tryal alſo of more nice and difficult Experiments. And till You have try'd them, do me the right to think that I deal not unſincerely with You.

The Reaſons of my having divers times recorded Experiments which You may have formerly met with, and perchance ev'n in Printed Books, I have elſewhere deduc'd in a peculiar Diſcourſe on that Subject: and therefore ſhall now only add, that by reaſon of my being as yet a ſtranger to the German Tongue, wherein the moſt and beſt Chymical Books are ſaid to be written, I may have ſet down divers

divers Chymical Experiments and Obſervations that are extant already in that Hermetical Language, (if I may ſo call it) without having had them from their Dutch Publiſhers, or ſo much as dream'd of their having been divulg'd by any man. I have likewiſe in my Preface to the Eſſays that you will meet with under the Title of Promiſcuous Experiments, given You an account why I have not refrain'd from mentioning divers things which may ſeem very ſlight, becauſe very obvious: And I have long had thoughts to inform You in an intire Diſcourſe to be written on purpoſe, why I think that ev'n the trivial, and therefore ſlighted, truths of Phyſiology ought not to be deſpis'd. And for my own part, I ſhall not ſcruple to confeſs to You, that I diſdain not to take Notice ev'n of Ludicrous Experiments, and think that the Plays of Boys may ſometimes deſerve to be the Study of Philoſophers: For as when we go a Hunting, though the flight of the Hare and the purſute of the Dogs be to us but ſport and recreation, yet the Beaſts themſelves are extreamly earneſt, the one to ſave his threatn'd life by flight, and the other to overtake his deſired Prey; ſo Nature acts very ſeriouſly in all the other things that we make ſports with, and is in very good earneſt, whether we Men be ſo or no.

Perhaps you will wonder, *Pyrophilus*, that in almoſt every one of the following Eſſays I ſhould ſpeak ſo doubtingly, and uſe ſo often, *Perhaps*, *It ſeems*, *'Tis not improbable*, and ſuch other expreſſions as argue a diffidence of the truth of the Opinions I incline to, and that I ſhould be ſo ſhy of laying down Principles, and ſometimes of ſo much as venturing at Explications. But I muſt freely confeſs to you, *Pyrophilus*, that having met with many things of which I could give my ſelf no one probable cauſe, and ſome things of which ſeveral Cauſes may be aſſign'd ſo differing, as not to agree in any thing unleſs in their being all of

 them

BIBL. CANT. ACAD.

them probable enough, I have often found ſuch Difficulties in ſearching into the Cauſes and Manner of things: and I am ſo ſenſible of my own Diſability to ſurmount thoſe Difficulties, that I dare ſpeak confidently and poſitively of very few things, except of Matters of fact. And when I venture to deliver any thing by way of Opinion, I ſhould, if it were not for meer ſhame, ſpeak yet more diffidently than I have been wont to do. 'Tis not that I at all condemn the Practice of thoſe Inquiſitive Wits that take upon them to explicate to us ev'n the abſtruſeſt Phænomena of Nature: For I am ſo far from cenſuring them, that I admire them when their Endeavours ſucceed, and applaud them ev'n where they do but fairly attempt. But I think 'tis fit for a man to know his own Abilities and Weakneſſes, and not to think himſelf oblig'd to imitate all that he thinks fit to praiſe. I know alſo that the way to get Reputation, is, to venture to explicate things, and promote Opinions: For by that courſe a Writer ſhall be ſure to be applauded by one ſort of men, and be mention'd by many others; whereas by the way of Writing to which I have condemn'd my ſelf, I can hope for little better among the more daring and leſs conſiderate ſort of men, ſhould you ſhew them theſe Papers, than to paſs for a Drudge of greater Induſtry than Reaſon, and fit for little more than to collect Experiments for more rational and Philoſophical heads to explicate and make uſe of. But I am content, provided Experimental Learning be really promoted, to contribute ev'n in the leaſt plauſible Way to the Advancement of it, and had rather not only be an Underbuilder, but ev'n dig in the Quarries for Materials towards ſo uſeful a Structure, as a ſolid body of Natural Philoſophy, than not do ſomething towards the Erection of it. Nor have my thoughts been altogether idle and wanting to themſelves, in framing Notions, and attempting to deviſe Hypotheſes, which might avoid the deficiencies

cies obſerv'd in other mens Theories and Explications: but I have hitherto, though not always, yet not unfrequently, found that what pleas'd me for a while, as fairly comporting with the Obſervations on which ſuch Notions were grounded, was ſoon after diſgrac'd by ſome further or new Experiment, which at the time of the framing of thoſe Notions was unknown to me, or not conſulted with. And indeed I have the leſs envy'd Many (for I ſay not All) of thoſe Writers who have taken upon them to deliver the Cauſes of things, and explicate the Myſteries of Nature, ſince I have had opportunity to obſerve how many of their Doctrines, after having been for a while applauded and even admir'd, have afterwards been confuted by the diſcovery of ſome new Phænomenon in Nature, which was either unknown to ſuch Writers, or not ſufficiently conſider'd by them. For I have found it happen as well to many others (that have publiſht their Opinions) as to me (who have been more private in my Gueſſes) in our Theories built on either too obvious or too few Experiments, what is wont to happen to the Falſifiers of Coyn: for as Counterfeit pieces of Money will endure ſome of them One Proof, as the Touch-ſtone, others Another, as Aqua fortis, ſome a third, as the Hammer or the Scales, but none of them will endure All proofs; ſo the Notions I mention (in which ſort I fear too great a part of thoſe hitherto extant may be compriz'd) may agree very fairly with this or that or the other Experiment, but being made too haſtily, and without Conſulting a competent number of them, 'tis to be fear'd that there may ſtill after a while be found one or other, (if not many) their Inconſiſtency with which will betray and diſcredit them.

I have notwithſtanding all this on ſome occaſions adventur'd to deliver my Opinion, not that I am very confident of being leſs ſubject to erre in thoſe particulars than in any of the others wherein I have refrain'd from interpoſing any

Conjecture, but becauſe I would manifeſt to You, that I ſcruple not to run the ſame venture with thoſe incomparably better Naturaliſts, that have thought it no diſgrace in difficult matters rather to hazard the being ſometimes miſtaken, than not to afford Inquiſitive Perſons their beſt Aſſiſtance towards the Diſcovery of Truth.

And becauſe, *Pyrophilus*, in the Reaſons and Explications I offer of Natural Effects, I have not for the moſt part an immediate recourſe to the Magnitude, Figure, and Motion of Atoms, or of the leaſt Particles of Bodies, I hold it not unfit to give You here ſome account of this Practice, not ſo much for the ſake of thoſe few Paſſages in my Eſſays that may be concern'd in it, as for that of many Learned men, eſpecially Phyſitians, whoſe uſeful Writings begin to be undervalu'd, and are in danger to be deſpis'd, by an Opinion taken up from the miſ-underſtood Doctrine of ſome eminent Atomiſts, as if no ſpeculations in Natural Philoſophy could be rational, wherein any other cauſes of things are aſſign'd than Atoms and their Properties. I conſider then, that generally ſpeaking, to render a reaſon of an Effect or Phænomenon, is to deduce it from ſomthing elſe in Nature more known than it ſelf, and that conſequently there may be divers kinds of Degrees of Explication of the ſame thing. For although ſuch Explications be the moſt ſatisfactory to the Underſtanding, wherein 'tis ſhewn how the effect is produc'd by the more primitive and Catholick Affection of Matter, namely, bulk, ſhape and motion, yet are not theſe Explications to be deſpis'd, wherein particular effects are deduc'd from the more obvious and familiar Qualities or ſtates of Bodies, ſuch as Heat, Cold, Weight, Fluidity, Hardneſs, Fermentation, &c. Though theſe themſelves do probably depend upon thoſe three univerſal ones formerly nam'd. For in the ſearch after Natural Cauſes, every new meaſure of Diſcovery does both inſtruct and gratifie the Underſtanding, though

I readily confeſs, that the nearer the diſcover'd Cauſes are to thoſe that are higheſt in the ſcale or ſeries of Cauſes, the more is the Intellect both gratify'd and inſtructed.

I think it therefore very fit and highly uſeful, that ſome ſpeculative Wits well vers'd in Mathematical Principles and Mechanical Contrivances, ſhould employ themſelves in deducing the chiefeſt Modes or Qualities of Matter, ſuch as are Heat, Cold, &c. and the States or conditions of it, (if we think fit to diſtinguiſh theſe from its Qualities) as fluid, firm, brittle, flexible, and the like, from the above-mention'd moſt primitive and ſimple Affections thereof. And I think the Common-wealth of Learning exceedingly beholden to thoſe Heroick Wits that do ſo much as plauſibly perform ſomething in this kind. But I think too, we are not to deſpiſe all thoſe Accounts of particular Effects which are not immediately deduc'd from thoſe primitive Affections of either Atoms or the inſenſible Particles of Matter, but from the familiar, though not ſo univerſal, Qualities of things, as cold, heat, weight, hardneſs, and the like. And perhaps it would be none of the leaſt advantages which would accrue to Naturaliſts from a ſatisfactory explication of ſuch Qualities by the moſt primitive and ſimple ones, that it would much ſhorten the explication of particular Phænomena: For though there be many things in Nature that may be readily enough made out by the Size, Motion, & Figure of the ſmall Parts of Matter, yet there are many more that cannot be well explain'd without a great deal of Diſcourſe, and divers ſucceſſive Deductions of one thing from another, if the purpos'd effect muſt be deduc'd from ſuch primary and Univerſal Cauſes; whereas if we be allow'd to take the Notions of Cold, Heat, and the like Qualities for granted, the explications and proofs may be much more compendiouſly made. He gives ſome Reaſon why Stones and Iron and all other heavy Bodies will ſwim in Quick-ſilver, except

Gold,

Gold, which will ſink in it, that teaches, that all thoſe other Bodies are *in ſpecie* (as they ſpeak) or bulk for bulk, lighter than Quick-ſilver, whereas Gold is heavier. He, I ſay, may be allow'd to have render'd a Reaſon of the thing propos'd, that thus refers the Phænomenon to that known Affection of almoſt all Bodies here below, which we call Gravity, though he do not deduce the Phænomenon from Atoms, nor give us the cauſe of Gravity, as indeed ſcarce any Philoſopher has yet given us a ſatisfactory Account of it. So if it be demanded, why, if the ſides of a blown Bladder be ſomewhat ſqueez'd betwixt ones hands, they will, upon the removal of that which compreſs'd them, fly out again, and reſtore the Bladder to its former figure and dimenſions, it is not ſaying nothing to the purpoſe, to ſay that this happens from the ſpring of thoſe Aerial Particles wherewith the Bladder is fill'd, though he that ſays this be not perhaps able to declare whence proceeds the Motion of Reſtitution, either in a Particle of compreſs'd Air, or any other bent ſpring.

And as for the Reaſons of things aſſign'd by Phyſitians, they muſt be moſt of them deſpis'd, unleſs we will allow of ſuch explications as deduce not things from Atoms or their Affections, but only either from ſecondary Qualities, or from the more particular Properties of Mixt Bodies. If a Phyſitian be ask'd why Rhubarb does commonly cure Looſeneſſes, he will probably tell you as a Reaſon, that Rhubarb is available in ſuch Diſeaſes, becauſe it hath both a Laxative vertue, whereby it evacuates Choler, and ſuch other bad humours as are wont in ſuch caſes to be the peccant Matter, and an aſtringent Quality, whereby it afterwards arreſts the Flux: But if you further ask him the Reaſon why Rhubarb purges, and why it purges Choler more than any other humour, 'tis ten to one he will not be able to give you a ſatisfactory anſwer. And indeed, not only the manner whereby Purgative Medicines Work,

bu-

but thoſe other Properties whereby ſome Bodies are Diuretick, others Sudorifick, others Sarcotick, &c. are not I fear ſo eaſie to be intelligibly made out as men imagine, and yet a skilful Phyſitian would juſtly think himſelf wrong'd, if the Reaſons he renders of things in his own Profeſſion were deny'd the Name of Reaſons, becauſe made without recourſe to Atomical Principles. And indeed, there are oftentimes ſo many ſubordinate Cauſes between particular Effects and the moſt General Cauſes of things, that there is left a large field wherein to exerciſe Mens Induſtry and Reaſon, if they will but ſolidly enough deduce the Properties of things from more general and familiar Qualities, and alſo intermediate Cauſes (if I may ſo call them) from one another. And I am the more backward to deſpiſe ſuch kind of Reaſons, becauſe I elſewhere declare, that there are Some (for I do not ſay, Many) things, as particularly the Origine of Local Motion, of which ev'n by the Atomical Doctrine no Phyſical Cauſe can well be render'd; ſince either ſuch things muſt be aſcrib'd to God, who is indeed the true, but the ſupernatural Cauſe of them, or elſe it muſt be ſaid, (as it was by the old Epicureans) that they did ever belong to Matter, which, conſidering that the Notion of Matter may be compleat without them, is not to give a Phyſical efficient cauſe of the things in Queſtion, but in effect to confeſs that they have no ſuch Cauſes. But of this elſewhere more.

In the mean time, that you may not be drawn away to undervalue ſuch Writers as I have been pleading for, nor think you ought to refrain from writing what occurs to you, though true and uſeful, unleſs you deduce it, or at leaſt can do ſo, from the Epicurean Notions, I ſhall here briefly repreſent to you, (what perhaps you will not hereafter think a deſpicable ſuggeſtion) that there are two very diſtinct Ends that Men may propound to themſelves in ſtudying Natural Philoſophy. For ſome Men care only

to

to Know Nature, others desire to Command Her: or to express it otherwise, some there are who desire but to Please themselves by the Discovery of the Causes of the known Phænomena, and others would be able to produce new ones, and bring Nature to be serviceable to their particular Ends, whether of Health, or Riches, or sensual Delight. Now as I shall not deny but that the Atomical, the Cartesian, or some such Principles, are likely to afford the most of satisfaction to those speculative Wits that aim but at the knowledge of Causes; so I think that the other sort of men may very delightfully & successfully prosecute their ends, by collecting and making Variety of Experiments and Observations, since thereby learning the Qualities and Properties of those particular Bodies they desire to make use of, and observing the power that divers Chymical Operations, and other ways of handling Matter, have of altering such Bodies, and varying their effects upon one another, they may by the help of Attention and Industry be able to do many Things, some of them very Strange, and more of them very Useful in humane life. When a Gunner or a Souldier employs Gun-powder, it is not necessary that he should consider, or so much as know, of what and of how many Ingredients (much less of what kind of Atoms) it is made, and the proportion and manner wherein they are mingled; but the Notice Experience gives him of the power of that admirable Concrete, as it is made up and brought to his hands, suffices to enable him to perform things with it, that nothing but their being common and unheeded can keep from being admir'd. The Physitian that has observ'd the Medicinal vertues of Treacle, without knowing so much of the names, much less the Nature of each of the sixty and odd Ingredients whereof it is compounded, may cure many Patients with it. And though it must not be deny'd, that it is an advantage as well as a satisfaction, to know in general how the Qualities

of

of things are deducible from the primitive Affections of the ſmalleſt parts of Matter, yet whether we know that or no, if we know the Qualities of this or that Body they compoſe, and how 'tis diſpos'd to work upon other Bodies, or be brought on by them, we may, without aſcending to the Top in the ſeries of Cauſes, perform things of great Moment, and ſuch as without the diligent Examination of particular Bodies would, I fear, never have been found out *à priori* ev'n by the moſt profound Contemplators. We ſee that the Artificers that never dream'd of the Epicurean Philoſophy, have accommodated Mankind with a Multitude of uſeful Inventions. And *Paracelſus*, who (beſides that he ſeems none of the moſt piercing and ſpeculative Wits) ſure had little recourſe to Atomical Notions, if he ever ſo much as heard of them, was able to perform ſome things that were truly admirable, beſides thoſe he vainly boaſted of; as may appear not only by what I elſewhere repreſent, but by what *Oporinus* himſelf (as ſeverely as he otherwiſe writes againſt his deſerted Maſter) confeſſes he ſaw of the ſtupendous cures which *Paracelſus* wrought with his famous *Laudanum*, (whatever he made it of.) But we need not go far to find a noble Example to our preſent purpoſe, ſince we ſee that the bare making of tryals with the Load-ſtone, and Irons touch'd by it, though the Experimentors were ignorant (as ſome fear we yet are) of the true and firſt Cauſes of Magnetical Phænomena, have produc'd Inventions of greater uſe to Mankind, than were ever made by *Leucippus*, or *Epicurus*, or *Ariſtotle*, or *Teleſius*, or *Campanella*, or perhaps any of the ſpeculative Deviſers of new Hypotheſes, whoſe Contemplations aiming for the moſt part but at the ſolving, not the encreaſing or applying, of the Phænomena of Nature, it is no wonder they have been more ingenious than fruitful, and have Hitherto more delighted than otherwiſe benefitted Mankind: I ſay Hitherto, becauſe though Expe-

rience warrants me so to speak now, yet I am not unwilling to think that Hereafter, and perhaps in no long time, when Physiological Theories shall be better establish'd, and built upon a more competent number of Particulars, the Deductions that may be made from them may free them from all Imputation of Barrenness. But of these things I otherwhere (though not as I remember in any of the following Essays) more fully discourse.

And therefore I shall now resume the Subject that occasion'd this long Excursion, and add to what I said in excuse of my venturing sometimes to deliver something as my Opinion in difficult or controverted cases, that I must declare to you, *Pyrophilus*, that as I desire not my Opinions should have more Weight with you than the Proofs brought to countenance them will give them, so you must not expect that I should think my self oblig'd to adhere to them any longer than those Considerations that first made me embrace them shall seem of greater Moment than any that I can meet with in opposition to them. For *Aristotle* spoke like a Philosopher, when to justifie his Dissent from his Master *Plato*, he said among other things, That for the sake of Truth, men (especially being Philosophers) ought to overthrow ev'n their own Tenents (Δόξειε δ' ἂν ἴσως βέλτιον εἶναι, καὶ δεῖν ἐπὶ σωτηρίᾳ γε τῆς ἀληθείας καὶ τὰ οἰκεῖα ἀναιρεῖν, ἄλλως τε καὶ φιλοσόφους ὄντας.) And though for a man to change his opinions, without seeing more reason to forsake them than he had to assent to them, be a Censurable Levity and Inconstancy of mind; yet to adhere to whatever he once took for truth, though by Accession of more light he discover it to be erroneous, is but a proud Obstinacy very injurious to Truth, and very ill becoming the sense we ought to have of humane frailties. And it ought to be esteem'd much less disgraceful to quit an Error for a Truth, than to be guilty of the Vanity and Perverseness of believing a thing still, because we once believ'd it. And certainly,

Ethic. Nicom. lib. 1. cap. 6.

certainly, till a Man is ſure he is infallible, it is not fit for him to be unalterable.

You will eaſily diſcern, *Pyrophilus*, that I have purpoſely in the enſuing Eſſays refrain'd from ſwelling my Diſcourſes with ſolemn and elaborate Confutations of other mens Opinions, unleſs it be in ſome very few Caſes, where I judg'd that they might prove great impediments to the Advancement of Experimental Learning; and even ſuch Opinions I have been wary of medling with, unleſs I ſuppos'd I could bring Experimental Objections againſt them. For 'tis none of my Deſign to engage my ſelf with or againſt any one Sect of Naturaliſts, but barely to invite you to embrace or refuſe Opinions as they are conſonant to Experiments, or clear Reaſons deduced thence, or at leaſt analogous thereunto, without thinking it yet ſeaſonable to contend very earneſtly for thoſe other Opinions which ſeem not yet determinable by ſuch Experiments or Reaſons. And indeed, to allude to our former Compariſon, I would endeavour to deſtroy thoſe curious but groundleſs ſtructures that men have built up of Opinions alone, by the ſame way (and with as little Noiſe) by which ſuch fantaſtical ſtructures as thoſe I mention'd to have ſeen at *Leyden* may be demoliſh'd. To deſtroy which 'twere needleſs to bring battering Engines, ſince nothing is requiſite to that effect but an encreaſe of Light. And Experience has ſhown us, that divers very plauſible and radicated Opinions, ſuch as that of the Unhabitableneſs of the Torrid Zone, of the Solidity of the Celeſtial part of the World, of the Blood's being convey'd from the Heart by the Veins (not the Arteries) to the outward parts of the body, are generally grown out of requeſt, upon the appearing of thoſe new Diſcoveries with which they are inconſiſtent, and would have been abandon'd by the Generality of Judicious Perſons, though no man had made it his buſineſs purpoſely to write Confutations of them: ſo true is that

Vulgar ſaying, that *Rectum eſt Index ſui & Obliqui.*

But when at any time, *Pyrophilus*, I have been induc'd to oppoſe others, as I have not deny'd my ſelf the freedom that is requiſite unto Loyalty to Truth, ſo I have endeavour'd to uſe that Moderation and Civility that is due to the perſons of deſerving Men. And therefore you ſhall find me not only in one Eſſay oppoſe an Author whom in another I applaude, but ſometimes you may find me commending a Writer in the very ſame Page, perhaps, where I am endeavouring to diſprove his Opinions: For I love to ſpeak of Perſons with Civility, though of Things with Freedom. Nor do I think it reaſonable, either that any mans Reputation ſhould protect his Errors, or that the Truth ſhould fare the worſe for his ſake that delivers it. And as for the (very much too common) Practice of many, who write, as if they thought, railing at a mans Perſon, or wrangling about his Words, neceſſary to the Confutation of his Opinions; beſides that I think ſuch a quarrelſome and injurious way of writing does very much miſbecome both a Philoſopher and a Chriſtian, methinks it is as unwiſe, as it is provoking. For if I civilly endeavour to reaſon a man out of his Opinions, I make my ſelf but one work to do, namely, to convince his Underſtanding: but if in a bitter or exaſperating way I oppoſe his Errors, I encreaſe the Difficulties I would ſurmount, and have as well his Affections againſt me as his Judgment: and it is very uneaſie to make a Proſelyte of him that is not only a Diſſenter from us, but an Enemy to us. And that which makes me the more diſlike the bitter way of diſputing which I am reprehending, is, that I have often obſerv'd, that though one of the Diſputants alone be at firſt in the fault, yet the other is moſt commonly drawn to ſhare in the Guilt, though to contract it he muſt imitate his Adverſary. For as a mad Dog by biting others is wont to make thoſe he bites run mad like himſelf, and do promiſcuous Miſchief;

Miſchief; ſo theſe ſo provoking Writers are wont to enrage thoſe they offend, and infect them alſo with their own virulent diſtemper. But, *Pyrophilus*, when I ſpeak of dealing reſpectfully with thoſe I diſſent from, I would be underſtood of ſuch as have well deſerved of Experimental Learning, or at leaſt been candid and ſober Inquirers after Truth. For, as I think that it would much diſcourage any prudent Perſon from venturing to communicate what he thinks he knows to the World, to find that an Error proceeding from humane Weakneſs, or the Darkneſs and Abſtruſity of things, ſeldom eſcapes being detected without being made matter of diſgrace or reproach to the Author: ſo on the other ſide, when vain Writers, to get themſelves a name, have preſum'd to obtrude upon the credulous World ſuch things, under the Notion of Experimental Truths, or even great Myſteries, as neither themſelves ever took the pains to make tryal of, nor receiv'd from any credible Perſons that profeſs'd themſelves to have try'd them; in ſuch caſes, I ſee not how we are oblig'd to treat Writers that took no pains to keep themſelves from miſtaking or deceiving, nay, that car'd not how they abuſe us to win themſelves a name, with the ſame reſpect that we owe to thoſe, who though they have miſs'd of the Truth, believ'd they had found it, and both intended to deliver It, and took ſome (though not proſperous) pains that they might convey nothing elſe to us.

I fear it will be requiſite, *Pyrophilus*, to tell you why in ſome of the following Eſſays you will meet with many Paſſages tranſcrib'd out of other Authors, and in ſome very few or none at all. And therefore to give you firſt a ſhort Account of the Particular mention'd laſt, I muſt mind you, that 'twas moſt ſuitable both to my Humor and Deſign to deliver only thoſe things wherewith my own Obſervations, or Tryals, or Thoughts, had furniſh'd me, without troubling you with the Repetition of thoſe things which had

had been deliver'd by others already; thoſe kind of Repetitions, unleſs they be made upon ſome ſuch grounds as we ſhall preſently mention, ſeeming to me to be as vainly as ambitiouſly affected by many Writers, and being deſervedly troubleſome to Judicious Readers, who can eaſily diſcern that they add much more to the bulk of Books than of Knowledge. But this notwithſtanding, *Pyrophilus*, I thought my ſelf oblig'd on ſome occaſions, for your ſake, to overcome my Natural Averſeneſs to ſtuff any Writings of mine with Paſſages tranſcrib'd from thoſe of others, partly for the Reaſons elſewhere inſiſted on, and partly for divers others. As Firſt, becauſe ſome Particulars are of that Strangeneſs, and of that Moment, that they need and deſerve to be verify'd by more than a ſingle Atteſtation. Next, becauſe according to the Greek Proverb, κοινὰ καινῶς, it is not properly to ſay over the ſame thing again, when the Obſervation, Experiment, or other Paſſage of an Author, is either illuſtrated or otherwiſe improv'd by the Repetition, or elſe apply'd to ſome purpoſe differing from that to which the Author brought it: That being applicable to many a ſingle Experiment or Obſervation which *Seneca* ſomewhere ſays, *Nulla Res conſummata eſt dum incipit*; And, *Etiamſi omnia à Veteribus inventa ſunt, hoc ſemper novum erit, Uſus, & Inventorum ab aliis Scientia & Diſpoſitio.* And thirdly, becauſe as the Planets and other Stars have (according to Aſtrologers) in their great Synods or Conjunctions, much more powerful, and ſometimes other Influences on the Air and ſome other ſublunary Parts of the World, than are aſcrib'd to one or two of them out of that Aſpect; ſo divers Particulars, which whilſt they lay ſingle and ſcatter'd among the Writings of ſeveral Authors were inconſiderable, when they come to be laid together in order to the ſame Deſign, may oftentimes prove highly uſeful to Phyſiology in their Conjunction, wherein one of them may ſerve to prove one part

Quæſtion. Natural. lib. 6. cap. 5. *Epiſtola* 64.

or

or circumſtance of an important Truth, and another to explicate another, and ſo all of them may conſpire together to verifie that Saying, *Et quæ non proſunt ſingula, multa juvant.* It may then I hope ſuffice to juſtifie me on this occaſion, that not appealing to other Writers as to Judges, but as to Witneſſes, nor employing what I have found already publiſh'd by them barely as Ornaments to imbelliſh my Writings, and much leſs as Oracles by their Authority to demonſtrate my Opinions, but as Certificates to atteſt Matters of fact, I may hope that their Teſtimonies will as well be illuſtrated by mine, as mine by their's, and that all of them may contribute to your better Information.

And if, *Pyrophilus*, you grant that upon theſe Conſiderations I have not done amiſs to apply to my purpoſe divers of thoſe things which I found deliver'd pertinently to them by thoſe Writers which I chanc'd to caſt mine Eyes on, I ſuppoſe you will not think I need to make you an Apology for my having made moſt uſe of the Paſſages of thoſe Writers which I ſuppoſe will be moſt difficult to be met with (ſuch as are many Books of Navigations and other Voyages) and eſpecially of French Books not yet tranſlated into Engliſh or Latin. And I think I ſhall leſs need to make an Excuſe for my having for the moſt part ſet down the Paſſages I recited in the Authors own Words, that being one of the readieſt ways I know to ſatisfie the Reader, and avoid injuring the Writer. And indeed, I have met with abundance of Quotations wherein the Tranſcriber doth ſo miſtake, or ſo miſ-repreſent the cited Authors Meaning, ſometimes out of Inadvertence, but ſometimes too I fear out of Indulgence to his own Hypotheſis, that if ever I ſhould be tempted to trouble the World with any of my thoughts, I would beſeech my Readers, not to look upon any thing as my Opinion or Aſſertion that is not deliver'd in the entire Series of my own

Words;

Words; leſt a Tranſcriber ſhould make me deliver thoſe things reſolutely and dogmatically, which I deliver but hæſitantly and conjecturally; and leſt I ſhould ſeem to ſet down thoſe things Poſitively as Proceſſes for whoſe ſucceſs I undertake, which I record but by way of Narrative.

For my ſo frequently mentioning what I have borrow'd from other Writers, or receiv'd from my friends, I expect to be excus'd by that of *Pliny*, *Benignum eſt (ut arbitror) & plenum ingenui Pudoris, confiteri per quos profeceris.* Though I have ſeen divers Modern Writers that ſo boldly uſurp the Obſervations and Experiments of others, that I might juſtly apply to them what the ſame *Pliny* annexes; *Scito enim, conferentem Authores me deprehendiſſe à juratiſſimis & proximis Veteres tranſcriptos ad Verbum, neque nominatos, &c.* If other Writers ſhould not prove more equitable (for I will not ſay more thankful) than ſuch as theſe, they would quickly diſcourage thoſe whoſe aims are not very noble and ſincere, from gratifying the Publick with Inventions, whoſe Praiſe and Thanks would be uſurp'd by ſuch as will not name them. But perhaps they would be more juſt if they reflected on what our Author further adds, *Obnoxii profectò animi & infelicis ingenii eſt, deprehendi in furto malle quam mutuum reddere, cum præſertim ſors fiet ex Uſura.*

And now I have ſaid this concerning the Paſſages I have borrow'd from other Authors, it will not be improper to add ſomething about thoſe I have declin'd to borrow. For you may poſſibly marvel, that in divers of the Hiſtorical parts of my Writings I have omitted ſuch Teſtimonies either of *Pliny*, *Solinus*, *Ariſtotle*, *Theophraſtus*, *Ælian*, or perchance ſome of the ancient Phyſitians themſelves (who yet, as more converſant with things, are uſually more credible) as ſeems very pertinent to my Diſcourſe, and fit to prove what I deſign. But when I ſhall come to entertain

you

you about Natural Hiſtory, I doubt not but to ſatisfie you with the Reaſons I ſhall offer you of this Practice. In the mean time, I ſhall only tell you in ſhort, that though I have a juſt reſpect for thoſe Great Names I have mention'd; yet the ſenſe I have of the difficulties I have found to make and relate an Obſervation accurately and faithfully enough for a Naturaliſt to rely on; and the occaſions I have had of looking into divers matters of fact deliver'd in their Writings, with a bold and an impartial Curioſity; have made me conclude ſo many of thoſe Traditions to be either certainly falſe, or not certainly true, that except what they deliver upon their own particular Knowledge, or with peculiar Circumſtances that may recommend them to my belief, I am very ſhy of building any thing of moment upon foundations that I eſteem ſo unſure, and much leſs upon the ſuſpected Paſſages that *Wecker*, *Paracelſus*, *Porta*, &c. abound with. And therefore (though I well enough know how much I impoveriſh my Diſcourſe by this Niceneſs) yet I do not think it fair to imploy that as an Argument to convince you that has not that operation upon me my ſelf. And I the rather take notice of my forbearing to make uſe of the Hiſtorical Traditions and Chymical or Magical Secrets that I meet in the above-mention'd Authors, or any other makers of Collections, unleſs the Narrative be (as I was ſaying) expreſly enough deliver'd upon the Writers Perſonal Knowledge, or that of ſome other credible Witneſs; not only becauſe I would give you an account why ſeveral of my Writings are unfurniſh'd with what moſt Readers look on as the richeſt Ornaments of other mens, but becauſe if this warineſs could be introduc'd, t'would be the moſt effectual way of perſwading men to write thoſe kind of Tracts I would recommend, Phyſiological Eſſays. For he that will confine himſelf ſo ſtrictly, will ſcarce be often tempted on

Physical Subjects, to write either Systems or Volumns.

Another thing, *Pyrophilus*, I must needs advertise you of in reference to the ensuing Discourses, which is, That besides those Deficiencies in point of Ratiocination which are due to my personal Disabilities, I have purposely let pass some Few (and but very Few) Inferences which I discern'd well enough not to be cogent, because I was willing to acquaint you upon some particular Occasions with all the Experiments then occurring to me, which I thought might contribute to the Illustration of the Subject in hand, though each of them apart did not irrefragably, nor indeed so much as strongly infer the Conclusion in order to which they seem'd to have been mention'd as Premisses. And this Practice I made the less scruple of, because I was willing to exercise thereby your Reasoning Faculty, and try how far you would discern the Tendency of several things, all of them pertinent enough to the Subject in hand, but not all of them concluding to the main design in order whereunto they were alledg'd. And I supposed that the things by me mention'd, though not conclusive, being yet Experimental, the mention of them, which in a strictly Logical way of reasoning must have been forborn, might well make you amends for the Exercise to which I intended they should put your Reason.

There remains yet one thing, *Pyrophilus*, of which I suppose you will expect I should give you an Account; and that is, why in the ensuing Essays I have mention'd divers Experiments which I have not plainly and circumstantially enough delivered. To satisfie you concerning which, I must represent to you, First, That though for your sake I have oftentimes, contrary to my Reason and Genius, deliver'd things, to make them more clear, in such a Multitude of words, that I now seem even to my self to have in divers places been guilty of Verbosity; yet in some other

other paſſages, treating of things which Uſe had render'd very familiar to me, I may have, to ſhun Prolixity, unawares ſlipt into the Contrary Extream. Secondly, There are ſome Mechanical Experiments wherein I have purpoſely omitted ſome manual Circumſtances, becauſe I was unwilling to prejudice ſome ingenious Trades-men, who make either a Livelyhood, or at leaſt a gain, by the ſale of the productions of ſuch Experiments. And I made the leſs ſcruple to conceal ſuch Mechanical Circumſtances, (if I may ſo call them) becauſe they were not neceſſary to the Phyſiological Knowledge of the Experiments: in naming of which my intention was to teach you rather Philoſophy than Trades. Thirdly, I mention'd ſome things but darkly, either becauſe I receiv'd them upon Condition of ſecrecy, or becauſe ſome ingenious perſons that communicated them to me, or others to whom I imparted them, do yet make, and need to make, a pecuniary advantage of them. Fourthly, And ſome things that, either having been the fruits of my own Labours, or obtain'd in Exchange of ſuch, are freely at my own diſpoſal, I have not yet thought fit ſo plainly to reveal, not out of an envious deſign of having them bury'd with me, but that I may be always provided with ſome Rarity to barter with thoſe Secretiſts that will not part with one Secret but in Exchange for another, and think nothing worth their deſiring that is known already to above one or two Perſons. And I think it very lawful to reſerve always ſome conceal'd Experiments by me, wherewith to obtain the ſecrets of others, which being thereby gained, the other (as being no longer neceſſary to the former end) may freely be communicated.

And think not, *Pyrophilus*, that the bare mention of an Experiment as having been performed, though the way of making it be conceal'd, is of no uſe, if the Relator of the

Experiment be a Perſon that may ſafely be credited: For it is ſomething to be aſſur'd that ſuch and ſuch things have been really perform'd, and conſequently are poſſible to be done, though we be not yet particularly acquainted with the means of performing them. And he tells you ſomething, that tells you upon his own Knowledge, that in ſuch or ſuch Bodies, or Ways of operating on them, conſiderable things of ſuch or ſuch a Nature are to be met with. And for my part, when I go a Hawking or Setting, I think my ſelf beholden to him that aſſures me that in ſuch a field there is a Covey of Partridges, though he does no more towards the giving me them. And it is obvious how much *Europe* is beholden to *Columbus* for the Detection of many Countries in *America* which were not diſcover'd by him, nor perhaps till long after his Death, becauſe he firſt inform'd us Knowingly that there were unknown Regions beyond that vaſt Ocean which ſevers the Old World from the New. But I begin to digreſs, and therefore ſhall proceed to tell you, That I am the leſs troubled at my Omiſſion of the circumſtantial parts of ſome Experiments, becauſe I think it will be much for your Advantage to try them over again your ſelf. And as I have taken care by the truth of the Experiments I have deliver'd to ſecure your ſucceſs, in caſe you try them aright; ſo I cannot be very ſorry that you ſhould in ſome Particulars have a kind of Neceſſity laid on you to exerciſe your own induſtry, and thereby encreaſe your Experience.

But beſides all that has been ſaid, *Pyrophilus*, I muſt freely confeſs to you, that there is one thing particularly relating to your ſelf, which has made me refrain from delivering in the enſuing Eſſays ſome of the chief Chymical proceſſes wherewith they might have been enrich'd. For not yet knowing with what ſerioufneſs you will addict your

your ſelf to promote experimental Philoſophy, nor what uſe you will make of what has been unveiledly communicated to you, I was ſomewhat unwilling that ſome things which had coſt me a great deal of pains ſhould yet fall into any man's hands that ſcorns to purchaſe Knowledge with ſome pains, and I was deſirous, in caſe you ſhall prove as induſtrious as I hope you will, to have ſomething by me to encourage and cheriſh your induſtry, which may be more ſuitable to your improved Knowledge. For I muſt confeſs to you, that in reference to the Chymical proceſſes extant in the following Diſcourſes, I look upon moſt of them but as trifles, not only in compariſon of thoſe things which a knowing Chymiſt might have deliver'd on the ſame ſubjects, but even in regard of divers proceſſes (not impertinent to thoſe diſcourſes) wherewith I my ſelf, (as little as I am a Pretender in theſe Matters) am not unacquainted: and perhaps I would have given to the following Treatiſes the Title of Trifles, inſtead of that of Eſſays, if I had not been afraid of diſcouraging you thereby, and if the Chymical part of them had been the chief thing wherewith I intended to acquaint you in them. But if the reception you give to what we have already written, prove ſuch as may encourage us to proceed, we may perhaps, if God be pleas'd to vouchſafe us Life and Opportunity, be invited to impart to you thoſe more conſiderable Chymical Experiments, which either the Communication of our friends or our own Labours have preſented us. For it will be much in the power of the Entertainment which theſe Papers ſhall meet with, to make them either the Beginning of our Labours of this nature, or the End. And in the mean time, I think I may venture to tell you, that, as inconſiderable as I have confeſs'd divers of the Chymical Proceſſes mention'd in theſe Eſſays to be, yet if ever you take the pains (as I hope you will) to w[illegible]

Experim[illegible]

Experimental Eſſays, and confine your ſelf to take as little upon truſt as I have done, you will perhaps be ready to believe, that ſometimes a ſhort Eſſay of this nature, not to ſay ſome one ſingle Experiment, may have coſt me more than a whole Treatiſe written on ſuch a Subject, whereon to be able without Diſcredit to write Books, it is almoſt ſufficient to have read many. And give me leave to add, that as in ſuch kind of Compoſures, oftentimes the enabling himſelf to give a conſiderable Advertiſement, or even Hint, may coſt the Writer more than the making of ſeveral Experiments; ſo it may be alſo more beneficial to the Reader than the Knowledge of them. For we muſt not always meaſure the Conſiderableneſs of things by their moſt obvious and immediate uſefulneſs, but by their fitneſs to make or contribute to the diſcovery of things highly uſeful. As, if it be true, what is reported by good Authors of the hazel Wand, or *Virgula divinatoria*, though the hazel Tree be much leſs conſiderable in reference to its fruit, or immediate productions, than a Peach-Tree, an Orange-Tree, or even an Apple-Tree; yet may it be made much more valuable than any of them, becauſe whereas they only preſent us with fruits, this may aſſiſt us to diſcover in latent Mines ineſtimable Treaſures.

I had almoſt forgot to advertiſe you, *Pyrophilus*, That whereas I have not been ſo ſollicitous as moſt Writers are wont to be, to ſwell the enſuing Eſſays with the Enumeration of the variõus Opinions and Arguments of Authors about the Subjects I treat of, or to adorn them with acute Sentences, fine Expreſſions, or other Embelliſhments borrow'd from eminent Writers; it has not been, becauſe I utterly diſlike the making uſe of thoſe paſſages in Claſſick or other Authors, that may either give (among the Admirers of thoſe Writers) ſome Authority to our thoughts, or very handſomely and Emphatically expreſs them.

them. For I remember, I have known it reprehended by Learned Men in *Epicurus*, That though he writ very much himſelf, he would not vouchſafe in his Writings to quote thoſe of other Men. And that I have not refrain'd from making uſe, now and then, of thoſe Philological Ornaments of Diſcourſe, when they readily occur'd to me, and appear'd neither impertinent nor prolix, may I hope ſuffice to keep me from being ſuſpected of the Vanity of thinking my ſelf above other mens aſſiſtance. But the reaſons of my ſo much declining to make uſe of other mens Authority, or Expreſſions, were chiefly theſe. Firſt, That the Weakneſs of my Eyes has this long time kept me from reading almoſt any Books, ſave the Scripture, with ſome Critical Expoſitions of it, and here and there ſome Portions of the Writings of thoſe that pretend to teach their Readers Experimental Matters: And the unfaithfulneſs of my Memory as to things of no great Moment, has made me forget almoſt all the little Philological and florid Learning I was formerly acquainted with. And really, *Pyrophilus*, as for the Books that treat of Natural Philoſophy, I am ſo ſenſible of the ſmalneſs of the Advantage which my Diſabilities have ſuffer'd me to make of them, that inſtead of being ambitious to appear a great Reader, I could be very well content to be thought to have ſcarce look'd upon any other Book than that of Nature. And in the next place, *Pyrophilus*, though I ignore not that by this plain and unadorn'd way of Writing, I unkindly deny my Eſſays many Embelliſhments which I might give them, and which perhaps you will think they do abundantly need; yet my frequent Diſtempers, Journeys, and other Avocations, not allowing me ſo much time as I deſir'd, to entertain you on Philoſophical ſubjects, I thought it more requiſite to ſpend thoſe confin'd hours in acquainting you with my own thoughts, ſuch as they are, than with thoſe of

other

other Men; unleſs (as I formerly intimated) I can ſome way or other more than barely recite what I recite of theirs. And you will eaſily pardon me the injury which for your ſake I do my own Reputation by this naked way of writing, if you, as well as I, think thoſe the profitableſt Writers, or at leaſt the kindeſt to their Peruſers, who take not ſo much Care to appear Knowing Men themſelves, as to make their Readers ſuch.

TWO

TWO
ESSAYS,
Concerning the Unſucceſsfulneſs
OF
EXPERIMENTS,

CONTAINING

Divers Admonitions and Obſervations (chiefly Chymical) touching that *SUBJECT*.

LONDON,

Printed for *Henry Herringman*, *Anno* 1668.

TWO ESSAYS

Advertiſement about the two following Eſſays.

THE Author of theſe Diſcourſes had inlarged them in this ſecond Edition with divers Obſervations and Experiments, but that he has made uſe of them already in other Papers belonging to his Sceptical or Doubting Naturaliſt.

THE FIRST ESSAY.

Of the Unſucceſsfulneſs of EXPERIMENTS.

I Am very ſorry, *Pyrophilus*, that to the many (elſewhere enumerated) difficulties which you may meet with, and muſt therefore ſurmount, in the ſerious and effectual proſecution of Experimental Philoſophy, I muſt add one diſcouragement more, which will perhaps as much ſurprize you as diſhearten you; and it is, That beſides that you will find (as we elſewhere mention) many of the Experiments publiſh'd by Authors, or related to you by the perſons you converſe with, falſe or unſucceſsful, (beſides this, I ſay) you will meet with ſeveral Obſervations and Experiments, which though communicated for true by Candid Authors or undiſtruſted Eye-witneſſes, or perhaps recommended to you by your own experience, may upon further tryal diſappoint your expectation, either not at all ſucceeding conſtantly, or at leaſt varying much from what you expected.

This Advertiſement may ſeem of ſo diſcouraging a nature, that I ſhould much ſcruple the giving it you, but that

I ſuppoſe the trouble at that unſucceſsfulneſs which you may meet with in Experiments, may be ſomewhatleſſened, by your being forewarned of ſuch contingencies: And that if you ſhould have the luck to make an Experiment once, without being able to perform the ſame thing again, you might be apt to look upon ſuch diſappointments as the effects of an unfriendlineſs in Nature or Fortune to your particular attempts, as proceed but from a ſecret contingency incident to ſome experiments, by whomſoever they be tryed.

But becauſe, *Pyrophilus*, the Advertiſement which I am about to give you may ſeem, as Paradoxical, ſo diſcouraging; it will be but reaſonable that I preſent you with ſome inſtances of the requiſiteneſs of it: which I ſhall the more willingly do, becauſe thereby I may not only evince the truth of it, but ſomewhat leſſen the deſpondency it is apt to produce, by letting you ſee, that though ſome of your Experiments ſhould not always prove conſtant, you have divers Partners in that infelicity, who have not been diſcouraged by it.

To make nice and curious diſtinctions of the ſeveral grounds and occaſions of the Unſucceſsfulneſs of Experiments, would perhaps prove a work of greater difficulty than uſe, and therefore I ſhall content my ſelf groſly to diſtinguiſh the cauſes of that unſucceſsfulneſs, into the particular or miſtaken properties of the Materials imploy'd about them, and ſome ſuch error committed in the handling of theſe Materials, as though it hinder the ſucceſs of the Experiment, is not eaſie to be diſcerned. Which therefore I mention, that I may diſtinguiſh theſe kind of Errors that I am now to conſider from thoſe more obvious ones, which proceeding barely from the unskilfulneſs of the Tryers of the Experiments, may be eaſily enough diſcerned, and either rectifi'd or avoided by a knowing Artiſt, or a perſon well vers'd and expert in making thoſe particular

cular Experiments, which through that unskilfulneſs may have miſcarried.

The Materials to be imploy'd about the Experiments we are conſidering may alſo admit of ſeveral diſtinctions; as into Natural and Factitious, Sincere and Adulterate, Simple and Compound, &c. But we ſhall likewiſe purpoſely forbear the inſiſting on any of theſe, and content our ſelves to caſt what we have to ſay on this part of our Theme, into a few and comprehenſive Obſervations.

And in the firſt place we will obſerve, that divers Experiments ſucceed not, becauſe they were at one time tryed with Genuine Materials, and at another time with Sophiſticated ones: and in this caſe it may be all one, as to the Event of the Experiment, whether the Materials wherewith it was ſucceſsfully try'd were ſophiſticated or not, if thoſe made uſe of in the latter tryal were of differing Qualities from thoſe employ'd in the former; becauſe it may very well happen, that ſophiſticated Bodies (as we may have occaſion to ſhew hereafter) by the addition of thoſe things, or by that deceitful way of preparation, whereby they have been ſophiſticated, may acquire an aptitude to produce ſuch effects, as had they not been adulterated, they would not have been fit to do. Now it is ſcarcely imaginable to him that has not been very converſant with the Drugs and Simples ſold in Shops, how generally they are adulterated by the fraudulent avarice of the Sellers, eſpecially if they be ſuch whoſe preciouſneſs may make their Sophiſtication very beneficial to them that practiſe it. It has been lately much complained of by ſome of the Cultivators of *Clover-graſs*, that of a great quantity of the Seed not any Graſs ſprung up; which not being imputable to the Soyl, nor the Sower, proceeds, as ſome Analogical obſervations make me ſuppoſe, from the effeteneſs (if I may ſo ſpeak) of the ſuperannuated Seed ſometimes ſold in the Shops. And upon this Subject I cannot conceal from

from you what was lately affirm'd to me by one of the eminenteſt and ſobereſt Chymiſts of *Amſterdam*, who was alſo an Indian Merchant, who aſſur'd me, that moſt of the Cinnamon and Cloves that is brought into theſe Weſtern Regions, is defrauded in the *Indies* of much of the fineſt and ſubtileſt Aromatical parts before it be ſent into *Europe*. And to give a more familiar Inſtance to our preſent purpoſe, you may be pleaſed to remember, *Pyrophilus*, that in one of the firſt of theſe Eſſays, we have made mention to you of great ſtore of living Creatures which we had obſerved in Vinegar; of the truth of which Obſervation we can produce divers learned and ſevere Witneſſes, who were not to be convinced of it till we had fully ſatisfi'd them by ocular demonſtration: and yet, *Pyrophilus*, there are divers parcels of excellent Vinegar, wherein you may in vain ſeek for theſe living Creatures: and we are now diſtilling ſome of that Liquor (which if we did not think to be of the ſtrongeſt and beſt ſort, we ſhould ſcarce think worth the being diſtill'd for Spirit) wherein nevertheleſs we can neither by Candle-light nor by day-light diſcern Any of thoſe little Creatures of which we have often ſeen Swarms in other Vinegars. Of ſuch fraudulent tricks as thoſe lately mention'd, I could eaſily give you divers Inſtances, if I were not afraid of teaching Fallacies by diſcovering them. But ſome are ſo notorious, or otherwiſe of ſuch a nature, as that it may be more uſeful than dangerous to mention them.

It is commonly known, that *Sublimate* is wont to be ſophiſticated with *Arſnick*: and how differing the effects of ſuch *Sublimate* may be from thoſe of that which is faithfully prepar'd, not only upon Metals, but (when *Mercurius Dulcis* and other Preparations are made of it) upon humane Bodies, they, and ſcarce any but they, who are acquainted with the noxious qualities of *Arſnick*, both to Metals and Men, can readily imagine. And indeed as for

Chymical

Chymical Preparations *Helmont* was not much in the wrong, when he affirm'd, There were ſcarce any, vulgarly ſold in ſhops, to be rely'd on as faithfully prepar'd. And for my part, I have ſo often met with Chymical Preparations which I have found unſincere, that I dare ſcarce truſt any, either in the adminiſtration of Phyſick, or ſo much as in the tryal of conſiderable Experiments, which either my own Furnaces do not afford me, or wherewith I am not ſupply'd by ſome perſon of whoſe skill and faithfulneſs I have a good opinion. The other day, having occaſion to uſe ſome Spirit of Salt, whereof I was not then provided, I ſent for ſome to a Chymiſt, who making it himſelf, was the likelier to afford that which was well made: but though I gave him his own rate for it, at the firſt Rectification even in a Retort, a ſingle pound afforded us no leſs than ſix ounces of phlegm; and afterwards being further rectifi'd in a high body and gentle heat, the remaining Spirit parted with a ſcarce credible quantity of the like nauſeous liquor, and after all theſe ſequeſtrations of phlegm was not pure enough to perform what we expected from it. Of which complaining to an excellent Chymiſt of my acquaintance, he ſent for Spirit of Salt to a very eminent Diſtiller of it, who gets much by his profeſſion, and paſſeth for a very honeſt man: but this Spirit, beſides its weakneſs, diſcover'd it ſelf to be ſophiſticated with either Spirit of *Nitre*, or *Aqua fortis*, which betray'd it ſelf by its peculiar and odious ſmell; whereas Spirit of Salt skilfully and ſincerely drawn, is commonly of a greeniſh colour, bordering upon yellow, and hath uſually a Peculiar, and ſometimes (as I can exemplifie to you in ſome of mine) a not Unpleaſing ſmell. And let me on this occaſion advertiſe you, *Pyrophilus*, that in divers caſes

Accipe pulverem Johannis de Vigo *propria manu paratam, quam alioquin admiſtus Minio eſt adulteratus, prout quale-cunque Medicamen Chymicum quod venale exſtat fraude plenum eſt.* Helmon. de Febr. c. 14.

Sunt nempe olea eſſentialia venalia, quæque magno ære penduntur, adulterata omnia atque ſingula, &c. Idem de Feb. c. 15.

'tis not enough to ſeparate the aqueous parts by Dephlegmation, as many Chymiſts content themſelves to do, but ſome Liquors contain alſo an unſuſpected quantity of ſmall corpuſcules of ſomewhat an earthy nature, which being aſſociated with the ſaline ones, do clog or blunt them, and thereby weaken their activity: And therefore ſuch Liquors to be well depurated require the being diſtill'd off, and that with a ſlow fire, that the dry fæces may be left behind in the bottom of the Glaſs. To ſatisfie ſome perſons that this Obſervation is not groundleſs, we have ſometimes taken of the better ſort of Spirit of Salt, and having carefully dephlegm'd it, remov'd it into lower Glaſſes, (that the leſs heat will ſuffice to make the Liquor aſcend) and having gently abſtracted the whole Spirit, there remain'd in the bottom and the neck of the Retort whence 'twas diſtill'd, ſo great a quantity of a certain dry and ſtiptical ſubſtance, for the moſt part of a yellowiſh colour, that it ſeem'd ſtrange to the beholders, that ſo clear a Spirit ſhould conceal ſo much of it: and we our ſelves ſhould have wonder'd at it too, had we not remember'd, that in what the Chymiſts are wont to call the Oyl or Rectifi'd Butter of *Antimony* made with *Sublimate*, the Liquor, though diſtill'd and very limpid, almoſt like fair water, conſiſts in great part of the very body of the *Antimony*: which we would here manifeſt, but that we elſewhere do it, and therefore chuſe rather in this place to take notice, that the Spirit of Salt after this ſecond depuration was ſo chang'd, that it ſeem'd to be a much nobler, and almoſt another Liquor than it was before.

But to return to our ſophiſticated Spirit: what differing effects would be produc'd by true Spirit of Salt, and that which is mixt with the Spirit of *Nitre*, he that knows the great diſparity in the operations of thoſe two Liquors, whereof (to mention now no other Inſtances) the former will precipitate Silver, when the latter has diſſolv'd it, may

eaſily

eaſily inform you. Which Inſtances I mention not as the conſiderableſt which may be produc'd on this Subject, but as the freſheſt in my memory.

In the next place, *Pyrophilus*, I obſerve, that even when the Materials imploy'd about Experiments are no way ſophiſticated, but genuine, and ſuch as Nature has produc'd them, or Art ought to prepare them, even then, I ſay, there may be a very conſiderable Diſparity betwixt Concretions of the ſame kind and name, and which paſs without ſuſpicion for bodies of perfectly the ſame nature.

This may, to you, *Pyrophilus*, ſeem a great Paradox; but perhaps upon examination it will appear a great Truth: which becauſe I am perchance the firſt, or one of the firſt, that has ſolemnly aſſerted, I hope I ſhall obtain your pardon if I inſiſt ſomewhat the longer upon the making it out. For though *Antimony* (and the like is to be underſtood of Quick-ſilver, Gold, Copper, Tin, &c.) is wont by almoſt all men without heſitancy to be look'd upon as being all of it of the ſame nature as well as denomination; yet he that will take the liberty to ſuſpect that they may be deceiv'd in that opinion, and then heedfully obſerve the differing progreſs and event of Experiments, may very well diſcern, that there is as well a difference in Minerals of the ſame kind, as there is in Vegetables and Animals of the ſame ſpecies. And as the White-roſe, the Red-roſe, and the Damask-roſe differ much from one another, though all three be Roſes; and as the ſowre and ſweet Orange are very differing betwixt themſelves, and both of them from the *China* Orange, though all be Oranges; and as the Hound, the Grey-hound, the Spaniel, the Tumbler, the Maſtiff, and the Water dog, &c. are very diverſly qualifi'd, though all of them be Dogs: ſo neither are all the parcels of *Antimony* to be met with in Mines or Shops of altogether the ſame Qualities, though all of them be Antimonial Concretes. There is indeed this difference

betwixt the variety to be obſerved in Vegetables and Animals, and that which is to be found in Minerals, That the former is wont to be more obvious to the Eye, and betray it ſelf by ſome difference to be obſerved, either in the ſize of the Creatures of the ſame kind, or in ſome peculiar ſhape or colour, by which 'tis eaſie for Nature conſpicuouſly to diſcriminate bodies that conſiſt of many diſcernably diſtinct parts; whereas Minerals appearing to the eye either to be perfectly ſimilar, as Metals, or at leaſt to conſiſt but of two or three diſtinct ingredients, as *Cinnaber*, and ſome other Mineral Concretions, the diverſity to be found betwixt Minerals of the ſame Denomination is hardly to be diſcerned, before Experience have diſcover'd it.

And on this Subject I conſider, that the womb (if I may ſo ſpeak) of a Mineral body is not always like that of an Animal, a place by a competent and peculiar involving fence ſecur'd from the intruſion of all bodies not of kin to that included in it: But a Mineral being generated in the bowels of the Earth, its womb is oftentimes acceſſible and open to other Mineral Juyces or Steams that paſs that way, though of never ſo differing natures from that of the more copious Mineral. Inſomuch that not only I have had the opportunity to obſerve (not without ſome wonder) Minerals of differing kinds, as *Marchaſites* and *Metals*, *Marchaſites* and *Stones*, (I mean *Stones* properly ſo call'd) *Salt* and *Sulphur*, and the like, blended in the ſame ſmall lump of matter; but I have ſometimes found in a great maſs of one ſort of Mineral, ſmall parcels of a Mineral of a quit differing kind perfectly inclos'd in the ſubſtance of the other. But to reſume what we were ſaying before, theſe intruding bodies (if I may ſo ſpeak) being coagulated, and perhaps ripened together with the former by length of time, are not eaſily either ſeparable, or ſo much as diſtinguiſhable at their firſt digging out of the ground, and much leſs after their

their Colliquation. For the ignorant or heedleſs Mineman aiming only at the obtaining a quantity of ſuch a Metal, or other Mineral, as may be vendible under ſuch a determinate name, has neither the deſign, nor perhaps the skill, to make nice ſeparations of the Heterogeneous bodies to be met with in his Oar, but melts ſo much of them as he can promiſcuouſly together, and then ſells them, not only to the Merchant, but the Chymiſt, for that Metal or Mineral whoſe outward form and properties (as colour, conſiſtence, weight, ſound, &c.) it has; though that Metal under whoſe name it paſſes, be indeed but the predominant Ingredient of the Lump, wherein divers other Minerals may in ſmall quantities lye concealed, and yet upon occaſion be diſcovered by exquiſite ſeparations, or diſcover themſelves by unexpected operations, when they meet with bodies fit to act on them, or diſpos'd to receive impreſſions from them.

I was lately viſited by an ingenuous Goldſmith of my acquaintance, who complain'd to me, that being wont to buy parcels of Gold brought in ſmall pieces, and as it were ſandy corpuſcles, from *Guinea*, or ſome Country of that Coaſt, though he found it upon all tryals very right Gold, yet was it ſo very pale, that few but expert Goldſmiths would meddle with it, as fearing it had been ſome ſophiſticated Metal; adding, that this exceeding paleneſs of it ſometimes reduc'd him to melt it with very high-colour'd Gold, or to heighten its tincture with that of Copper, to bring it to the colour of ordinary Gold.

The probability of this may be prov'd by what is related by Monſieur *Flacourt*, Governor of the French Plantation in *Madagaſcar*, who in his newly publiſh'd Hiſtory of that Iſland, ſpeaking of the Metals of it, ſays, *Il y a bien, &c.* that is, There is certainly Gold among the Inhabitants of *Madagaſcar*, which has not been brought thither by Foreign Ships: for 'tis not poſ-

Hiſtoire de Madagaſcar, c. 37.

ſible that ſuch Ships ſhould have left them ſo much of that Metal as they have; and beſides, it is of a differing nature from that of *Europe*, which they call in this Country *Voulamene Voutroüa*. He adds, that this Gold, which they call Gold of *Malacaſſe*, is pale, and is not worth above 10 Crowns, (or about 50 ſhillings) an ounce; alſo, that the *Negroes* affirm, that there are many Mines of it in the Country where it was formerly digg'd; that there is three ſorts of it differing in fineneſs from each other, and diſcriminated by the Natives by three peculiar names. But that which he adds as moſt conſiderable, is, *That Malacaſſean Gold is of ſo very eaſie a fuſion, that it is almoſt as eaſily melted as Lead*; whereas we here find the Gold we deal with to require conſiderably ſtrong fires, and are wont to caſt in *Borax* to facilitate the fuſion.

Having upon occaſion had the Curioſity not long ſince to viſit ſome Mines of Lead, and other Metals, I find, that there is a great difference, and diſcernable even to the eye, betwixt the ſeveral Oars; for inſtance, of Lead, ſome of which I can ſhew you ſo like Steel, and ſo unlike common Lead-Oar, that the workmen upon that account are pleaſed to call it Steel-Oar, which being of more difficult fuſion than ordinary, they are wont to mix it with other Oar, which they call Firm-Oar, to facilitate the melting of it. And I likewiſe took notice of an Oar, which for its aptneſs to Vitrifie and ſerve the Potters to glaze their earthen Veſſels, the Miners call Pottern-Oar, and ſell it (at leaſt where I ſaw it digg'd up) dearer than other Oar, from which it differs both viſibly enough, and as the workmen affirm, in divers other (and thoſe leſs obvious) Qualities; and yet all theſe Oars after fuſion do paſs indiſcriminately under the name and notion of Lead. In which therefore it is no wonder that ſeverer Inquirers find a great deal of diſparity. I remember I did not long ſince cauſe ſome Lead-Oar to be try'd, which being the moſt promiſing

promiſing that ever I ſaw, made me ſuppoſe it might contain ſome conſiderable quantity of Silver: but though it prov'd ſo rich in Lead as to yield after the rate of ſeventy pound in the hundred, yet one of the moſt expert Artiſts in *Europe* could not extract one grain of Silver out of it; whereas the Lead of very many Mines, being skilfully examin'd, will leave behind it upon the Teſt a proportion of pure Silver. And though this quantity of Silver be not conſiderable enough to make ſuch Mines as yield it paſs for Silver-Mines, (or, as we are wont to call them, Mines-Royal) becauſe the Silver will not quit the coſt of extracting it; yet ſuch Mines, though they paſs but for Lead-Mines with the Metalliſt, may appear to be mixt Mines to the Naturaliſt, who may meet with divers Experiments, wherein the little Silver that is in them may make their Lead operate differently from that of thoſe Oars which are wholly deſtitute of Silver.

And as this diſparity is diſcernable in Lead-Oars, ſo it may well be ſuppoſed that the like would be diſcovered in the Oars of other Metals, if they were but purpoſely and skilfully examin'd. On which Subject I remember, that a very experienc'd perſon in theſe affairs, and otherwiſe very candid and ſober, was lately very deſirous I ſhould procure him ſome Tin-Oar, alledging, that he had met with a ſort of it, which after a long digeſtion in Lixiviate Liquors, afforded him a very conſiderable proportion of the richer Metals, inſomuch that having a large quantity of that Oar, and finding the Experiment on it to ſucceed conſtantly, he promiſed to himſelf a vaſt Income by it: But when that ſtock of Oar was ſpent, the next that he procured, though with great carefulneſs manag'd as the former, would by no means be brought to afford either ſo conſiderable a benefit, or ſo much as any at all. Which brings into my mind, that having once bought a parcel of block Tin, (as the Trades-men call that which is of the moſt

moſt pure or unmixt, and as yet unwrought) I was deſirous to try if I could not make a Menſtruum to diſſolve it in ſuch manner as *Aqua fortis* diſſolves Silver, and *Aqua Regis* Gold; becauſe Chymiſts are wont to complain, that though they have a Menſtruum or two that will diſſolve crude Tin, yet they want one that will keep it diſſolv'd, and will not, which *Aqua fortis* will, let it fall into a Calx. Having therefore (by a way that I elſewhere teach) prepar'd ſuch a Liquor as was deſir'd, I evaporated a Solution of the fore-mentioned Tin, and ſetting it to ſhoot, found, ſomewhat to my wonder, that the Cryſtals it afforded were not at all like any kind of Vitriol, but broad, flat, and exceeding thin, juſt like thoſe of Silver. Whereupon for further tryal having examin'd this Salt by the Tongue, we found not that it had any ſuch taſte as the skilfully made Calx of Tin in Spirit of Vinegar, (wherein 'tis not Every Calx of *Jupiter* that is ſoluble) which (the laſt time we try'd) ſeem'd to us to have, as it were, a chalybeate taſte, but ſuch an exceſſive bitterneſs as may be met with in the Cryſtals of Silver made with *Aqua fortis*: Finding alſo this further reſemblance betwixt the Salts of theſe two Metals, that they did both of them preſently dye upon the nails and skin a blackneſs that could not in a ſhort time be waſh'd off: we ſhould have ſuſpected, that the Menſtruum had exalted the Metal diſſolv'd in it to a greater cognation to Silver; but having afterwards proſecuted the ſame Tryal with the ſame Menſtruum, and another parcel of block Tin, (the former being caſually loſt) this Metal, though bought very ſoon after the other, and, as I remember, at the ſame place, made us conclude, that the event of our tryals proceeded from our having lighted upon a lump of Tin that was of a peculiar Nature.

I remember alſo that a while ſince a learned and inquiſitive friend of mine found in his Land a parcel of Oar, part of

of which he ſhewed me, and ſome of which I can ſhew you, but have not yet made tryal of it, which ſeem'd to me, among others that look'd upon it, to be Copper-Oar, and which did indeed after fuſion yield very good Copper; but the perſons to whom he committed the examination of the Mine, being very inquiſitive, and extraordinarily skilful, they did (as one of themſelves immediately after confeſs'd to me) find in that Oar, beſides the Copper, no inconſiderable quantity of Silver; and in that Silver (having diſſolv'd it in *Aqua fortis*) a conſiderable proportion of Gold.

But to detain you no longer on this Subject, give me only leave to ſtrengthen the Paradox I have propoſed by the authority of that great and candid Chymiſt *Baſilius Valentinus*, who ſpeaking of Antimony, after he hath told us that there are ſeveral kinds of it, and eſpecially two, the one more Mercurial, and of a Golden property, witneſſed by the ſhining ſtreaks or beams it abounds with, the other more full of Sulphur, but deſtitute of the golden nature that inriches the former, adds, that there is ſuch a different goodneſs betwixt the ſeveral ſorts of Antimony, as there is betwixt the ſeveral ſorts of Fleſh or Fiſh, which, though agreeing in name and, if you pleaſe, in nature, do exceedingly differ in point of goodneſs. Which brings into my mind the great difference which I have found, even viſible to the eye, betwixt the ſeveral ſorts of Antimony; and makes me alſo remember, that the other day being by an excellent Chymiſt ſhew'd a parcel of Antimony as a rarity, upon the ſcore of the variouſly-colour'd Sulphur wherewith it was conſpicuouſly inrich'd, the poſſeſſor of it ſoon after imploy'd it to make Butter of Antimony: but though he were very expert in that kind of diſtillation, yet inſtead of the Liquor he expected, upon the approach of a gentle fire, he found the neck and body of his Retort lin'd with an Antimonial Cinnaber, (or at leaſt a red ſubſtance, by him concluded

concluded to be Sulphur) at which being ſurprized, he was pleaſed to withdraw his fire till he had acquainted me with this accident, and in the yet unbroken Retort ſhew'd me the Cinnaber, which is not wont (as you know) to ariſe till after the Butter of Antimony is come over, and the remaining matter be urg'd with a vehement fire. And 'tis perhaps to the undiſcern'd difference of Antimonies that we may ſometimes aſcribe that contingency, which we have divers times had occaſion to take notice of in the making of Antimonial Cinnaber: for though in our Furnaces it hath been very ſucceſsfully made, yet not only we have afterwards fail'd of making it, but we have ſeen much more expert Chymiſts, and who becauſe of the high value they do (not undeſervedly) place upon that Medicine, imploy themſelves oftner than we in making it, divers times unſucceſsfully attempt the preparing it. And it may be perhaps alſo from ſome diverſity either in Antimonies or Irons, that eminent Chymiſts have (as we have obſerved) often failed in their endeavours to make the Starry Regulus of Mars and Antimony. Inſomuch that divers Artiſts fondly believe and teach (what our Experience will not permit us to allow) that there is a certain reſpect to Times and Conſtellations requiſite to the producing of this (I confeſs admirable) body. Upon which Subject I muſt not omit to tell you, that a while ſince an induſtrious acquaintance of ours was working on an Antimony, which unawares to him was, as we then ſuppos'd, of ſo peculiar a nature, that making a Regulus of it alone (without Iron) the common way, (for his manner of operation I enquir'd of him) he found, to his wonder, and ſhew'd me his Regulus adorned with a more conſpicuous Star than I have ſeen in ſeveral Stellate Reguluſſes of both Antimony and Mars. Yet I dare not be too confident that this depended only upon the peculiar nature of that Antimony, becauſe ſince that, my own Laboratory has afford-

ed

ed me divers ſuch parcels of Regulus without Mars (ſome of which I have yet by me very fairly ſtellated) which though made of Antimony that ſeem'd (by its various colours) to be more rich than ordinary in Sulphur; yet in regard the Antimony did not conſtantly afford a Starry Regulus, though by the ſame perſon order'd as near as could be after the ſame manner, it did not ſo clearly appear to me, whether the differing event of the ſeveral Tryals proceeded from the peculiar nature of this or that parcel of Antimony, or from ſome odd and ſcarce diſcoverable circumſtance in the management of the operation. But in either caſe, the mention of theſe uncertain Events will properly enough belong to our preſent Diſcourſe.

As in Antimony, ſo (as I intimated above) in divers other Minerals a conſiderable diverſity may be obſerved: & I remember I was lately preſented with a piece of a Mineral, which to me, as well as to the reſt who look'd on it, ſeem'd to be an ordinary and worthleſs Marchaſite; and yet a Dutch Merchant (a skilful Mineraliſt) who was the poſſeſſor of it, was very induſtrious to procure a greater quantity thereof, having in ſome of it, on which he had made Tryals, found a rich proportion of pure Gold. And the ſame Gentleman whoſe Copper-Oar I formerly mention'd, digging for more of that Oar, found lately a quantity of red Earth, which by knowing Mineraliſts was gueſt to be but Bolus, and indeed looked very like it; but being melted with Regulus Martis Stellatus by a skilful Tryer of Metals, it divers times richly recompenced the Examiners curioſity, by affording him many grains of fine Gold: and though I doubt whether this Gold proceeded from the Bolus, or the Regulus melted with it, yet however it may ſerve for an inſtance to ſhew that ſome Mineral bodies, which paſs without diſpute for Minerals of ſuch and ſuch a preciſe nature, may have lurking in them Minerals of a quite other nature, which may manifeſt

themſelves in ſome particular Experiments, (wherein they meet with proportionate Agents or Patients) though not in others.

That the Talck which is wont to be employ'd about Coſmeticks is of very difficult Calcination, is ſo known a thing to thoſe that have tryed to calcine it, that I have met with good Chymiſts that have looked on all the Calces of Talcks but as Impoſtures. Nor indeed have we calcin'd Venetian Talck without ſome length of time, and much violence of heat. But among many ſorts of Talck we have here in *England*, there is one which a moderate fire will in leſs than an hour reduce into a ſnow-white Calx, of which I had lately a parcel by me; and ſome days ſince I met with another ſort of Engliſh Talck which I could ſuddenly calcine even with the flame of a candle. And my experienc'd friend Dr. *K.* aſſures me, that out of a German Talck he met with, he did by digeſting it in a ſtrong Solution of Alcalizate Salts ſeparate pretty ſtore of good Gold, and might have made it a very gainful Experiment, if all the Talck growing in the ſame place had been of the ſame richneſs. The like almoſt has been affirmed to me by a Gentleman of Eminency, who told me, That from a certain Talck he had out of *Norway*, he had once drawn a pretty quantity of very good Gold: and it ſeems indeed, that though ſome have been pleaſed to laugh at all attempts of ſequeſtring any thing from any kind of Talck; yet ſome parcels of that Mineral afford good ſtore of a Tincture, which may for ought I know be of a golden nature. For I remember I have met with a kind of darkiſh-colour'd Talck (whereof I can yet ſhew you a piece) which when I caſt but into *Aqua Regis*, the Menſtruum manifeſtly work'd upon it, and diſſolv'd its colour'd parts in ſuch plenty, that the filtrated Solution paſs'd without ſuſpicion among divers knowing Naturaliſts to whom I ſhew'd it, for a fair Solution of Gold.

Paracel. *de Mineral. Tract.* 1.

Paracelſus

Paracelſus himſelf reckons four kinds of Talck, Red, White, Black, and of that colour which his Interpreter tranſlates Luteous: and perhaps each of theſe colours compriſes ſeveral kinds of that Mineral. And therefore that Mineraliſt did not amiſs when he added in the ſame Diſcourſe, after he had mention'd great variety of Marchaſites, Stones, and other Minerals, *Sed & hoc verum eſt, in terra multa adhuc condi, quæ mihi incognita ſunt, ſed eadem nec alii norunt. Certum ſiquidem eſt, progreſſu temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo noſtrûm nedum unquam ſomniavit.* Paracel. ibidem.

'Tis vulgarly known, that there is a great difference between Vitriols that are reputed to be meerly of the ſame Metal. And not to mention thoſe Vitriols that I have either made or ſeen, of leſs uſual colours; nor to take notice of the Veins, Slate, and even looſe Earth, impregnated with Coperas that I have had: to paſs by all this (I ſay) as for thoſe Vitriol Stones whereof we in *England* are wont to make our Vitriol, I have ſeen at the chief work where Coperas is made ſo great a variety of them, (divers of which I have yet lying by me) that I could ſcarcely believe the workmen when they affirmed them to be all Coperas Stones, and cannot but think it both very likely, that ſome of them contain other Mineral ſubſtances beſides Vitriol, and very poſſible that the ſaline parts of thoſe ſtones upon their ſolution by the Rain, may work upon thoſe other ſubſtances formerly concoagulated with them, and thereby imbue ſome parcels of the Vitriol made of them with qualities other than are eſſential to the nature of Vitriol, or belong ordinarily to it.

That there is alſo a difference betwixt thoſe bodies that paſs under the general name of common Salt, cannot but be obvious to any Chymiſt that hath occaſion to make accurate tryals on that Subject. And as for thoſe Con-

cretes that pass under the name of Salt-peter, there is probably no small disparity among them: for besides the difference which we have observed and which is obvious enough betwixt good English Nitre, and that which is brought us over from *Barbary*, (which before it is much refin'd abounds very much with an adventitious Salt that tastes much like Sea-salt) besides this I say, those that do use both good European & good East-Indian Salt-peter assure me, they find much difference betwixt them, and give the preference to the latter: and indeed I have often thought I discern'd a considerable difference in the operations of several kinds of Salt-peter even after purification: and probably that sort of Salt-peter which near *London* an ingenious man of my acquaintance does sometimes (but cannot always) make, chiefly out of Sea-salt, hath some differing qualities from that which is drawn the common way out of the Earth. And indeed Salt-peter being but a kind of *Sal terræ*, generated in very differingly-qualifi'd parcels of Earth, may probably receive divers qualities from the particular soyl wherein it grows, though these qualities lye concealed and unsuspected under the wonted exterior appearance of Nitre. Which consideration brings into my mind what was lately told me by a very ingenious Gentleman concerning one of the eminentest of our *London* Physitians, who was wont, as this Confident of his assured me, as an excellent secret, to imploy in some of his choice Remedies that peculiar Salt-peter which he had drawn out of the Earth digg'd up in Church-yards.

And such kind of differences would probably in other Mineral bodies be taken notice of, if mens prepossessions did not make them ascribe the variations they meet with in their Experiments, rather to any other cause than the unsuspected difference of the Materials imploy'd about them.

Nor is it only, *Pyrophilus*, among Mineral Bodies of the same

ſame name that ſuch a diverſity is to be found, but if narrowly look'd into, 'tis very probable that a greater diſparity may be diſcovered both among Vegetables and Animals, reputed of the ſame nature, than hath been yet taken notice of. Herbariſts indeed have exercis'd a commendable curioſity in ſubdividing Plants of the ſame denomination, and few Naturaliſts ignore that there are (for inſtance) many ſorts of Roſes, and of Apples, which differ widely betwixt themſelves, as we ſee the difference betwixt the Red-roſe and the White, betwixt the Crab, the Pippin, and the Pear-main. But beſides theſe differences which are obvious enough to be Regiſtred by Botanick Authors, there may be more undiſcern'd ones (which yet may be conſiderable ones) betwixt the Individuals of the ſame ultimate ſubdiviſion of Plants, ariſing partly from the temperature of the air, which makes (for example) Senna growing in *England* to differ much from that which is denominated from *Alexandria*; partly from the nature of the ſoyl, as is obvious in the change produced in wild Simples tranſplanted into Gardens; and partly from many other cauſes which we have not now leiſure to inſiſt upon. But we ſee oftentimes, that one Roſe much differs from another of the ſame kind, and one Pear-main from another Pear-main. To which we may add, that the upper cruſt or ſurface of the Earth being impregnated with ſubterraneal exhalations of ſeveral ſorts, and tempered with variety of Juyces, it may very poſſibly be, that ſome particular Plant may attract ſome ſuch Juyce out of a determinate ſpot of ground, as may give it Exotick qualities, and make it differ even from the neighbouring Plants of the ſame kind. To which purpoſe I remember, that travelling divers years ſince from *Geneva* towards *Italy*, I was in my paſſage through *Switzerland* by a Gentleman of thoſe parts (whoſe brother had been formerly my Domeſtick) invited to his Caſtle, and entertained among other things with a ſort of

Wine

Wine which was very heady, but otherwiſe ſeem'd to be Sack; and having never met with any ſuch Liquor during my long ſtay in thoſe parts, I was inquiſitive to know whence it was brought: and being anſwered that it grew amongſt thoſe Mountains, I could not believe it, till they aſſur'd me, that growing on a little ſpot of ground whoſe entrails abounded with Sulphur, it had from the ſoyl acquired its inebriating property, and thoſe other qualities which made it ſo differing from the Wine of the reſt of the Vineyards of that Country. And now I mention Wine, give me leave, *Pyrophilus*, to put you in mind of taking notice what a great change is made in that Liquor, when upon the receſs of the ſpirits and more volatile ſulphureous parts, or elſe the new texture they make with the others, it degenerates into Vinegar, and yet how little either diminution of quantity or any other alteration doth appear upon this change to the beholders eye. And though no body is like to loſe an Experiment by miſtaking Vinegar for Wine, becauſe both thoſe Liquors and the changes of them are ſo familiar unto us, and becauſe we are wont to taſte each of them before we imploy it; yet who knows what changes there may be in other Bodies with whoſe alterations we are unacquainted, though the Eye, which is oftentimes the only Senſe employ'd about judging of them, diſcern no change in them? as may daily be obſerved in the ſuperannuated ſeeds of Plants, which after their having been kept long beyond their due time, loſe all their germinating power without loſing any of their obvious qualities. And here let me further obſerve to you, that Urine is made much uſe of, not only by Dyers, but ſeveral other Trades-men in divers operations (ſome of which we may elſewhere have occaſion to treat of) belonging to their profeſſions. Now theſe men being wont indiſcriminately to employ Urine, without examining whether it be rich in Salt or not, and how long it hath been kept,

kept, it may not be impertinent to take notice that Chymists, who have occasion to distil it often in great quantities, assure me that they find a notable disparity betwixt Urines, that of healthy and young men abounding much more with volatile Salt than that of sickly or aged persons; and that of such as drink Wine freely being much fuller of spiritous and active parts than that of those whose drink is but Beer or Water. But because the differing strength of Urines, though it be very probable, is not so easily to be satisfactorily made out, we shall rather insist on this other Observation confirmed to us by Experience, which is, that though Trades-men are often wont to boil such and such things indifferently in any Urine, as if it were all one how new or stale it is, they may sometimes thereby commit considerable errors. For recent Urine, wherein the saline parts are yet intangled among the rest, will suffer it self to be boil'd above one half or two thirds away, without the avolation of its volatile salt and spirits. Whereas Urine that has been divers weeks kept is liable to a Putrefaction, whereby the Cement (if I may so speak) of the Ingredients that it consists of, perishing, or some change of texture occasioning their disjunction (if not also concurring to produce them) the component parts fall asunder, and the saline Particles extricating themselves from the rest, will even upon a very gentle heat (as tryal made on purpose has inform'd us) flye away, and leave a phlegmatick and unactive Liquor behind them. In confirmation whereof I must acquaint you, *Pyrophilus*, with what lately befel me in reference to the distillation of Urine: for having caused some of it to be buryed in earthen Vessels in a dunghil to be there putrifi'd, for five or six weeks, I was by divers occasional Journeys kept from employing it, till it had layn there between four and five months; and observing, when I caus'd it to be taken out, that the covers of the vessels had not been, by him I employ'd to put them

in,

in, well luted on, and beſides were in ſome places crackt, I ſuſpected that the Heat of the Dunghil had not only looſened the ſaline parts of the Liquor, but driven them away: and accordingly by diſtilling it in a very gentle heat, and in a very high Cucurbit, we obtain'd inſtead of an active and ſaline ſpirit, a languid and nauſeous phlegm. And how great odds there may be betwixt ſome Experiments made with recent and putrifi'd Urine, may be eaſily conceiv'd by him who knows what operations Salts have in the buſineſs of Colours, and is acquainted with their efficacy in thoſe other Mechanical Experiments wherein Urine is wont to be employ'd. But I fear I have dwelt too long upon this Theme, and therefore I ſhall proceed to the next.

And in the third place, *Pyrophilus*, I ſhall obſerve to you, that there is a great difference to be found among many things prepar'd by Art, that paſs under the ſame general name: which difference may proceed partly from that which we have already obſerv'd to be found in the Materials of which ſuch factitious Bodies are made, and partly from the way us'd in preparing them. To theſe heads many particulars may be reduc'd: But we ſhall at preſent reſtrain our ſelves to the mention of two ſorts of prepar'd bodies, namely, of ſuch as are not putrifi'd and exalted enough, and of ſuch as are ſo too much.

And to begin with the firſt of theſe; it is very certain, that divers Chymical Experiments delivered by ſober Authors have been believed falſe, only becauſe the Menſtruums or other Materials emyloy'd in the unſucceſsful tryals of them were not as highly rectifi'd, or otherwiſe as exquiſitely depurated, as thoſe that were us'd by the Deliverers of thoſe Experiments; ſo that oftentimes the fault of a bad Menſtruum is injuriouſly imputed to a good Artiſt. That experienc'd Chymiſt *Van Helmont*, in his Paradoxical Treatiſe of the Stone, endeavors (as we have elſe-

elſewhere mention'd) to explicate the manner of its being generated by the Coagulation immediately enſuing upon the mixture of the two volatile Spirits of Urine and of Wine. This noble Experiment has been by many unſucceſsfully try'd, and has been therefore by them diſcredited as a Chymical fiction: and indeed the firſt, and I think the ſecond time we attempted to make that Coagulum, we found nothing at all of any ſuch thing as we expected upon the confuſion of the two fore-mentioned Liquors, which though never ſo much ſhaken, and afterwards permitted to reſt, did never in the leaſt meaſure coagulate, which made us long ſuſpect the Experiment; till at length our favourable thoughts of that expert Chymiſt, making us think it poſſible that the Spirits we employ'd had not been ſufficiently exalted, we dephlegmated ſome by more frequent, and indeed tedious Rectifications (which yet prov'd but neceſſary) and then were ſatisfied by more accurate tryals, that *Helmont* had not miſ-inform'd us.

So likewiſe the ſame Author in his Treatiſe *de Peſte* much extolling, as a friend to the Stomach, the Entrails, the nervous parts, and even the Head, the Tincture or Solution of Amber made with ſpirit of Wine (which Medicine is indeed no ignoble one when adminiſtred to Conſtitutions that can well bear the heat of it) divers Phyſitians and Chymiſts have attempted the preparing of this Tincture with ſuch bad ſucceſs, that they have given out, that either *Helmont* delivered what was not true, or conceal'd ſome conſiderable Circumſtance of the Proceſs.

Whereas having digeſted ſufficiently dephlegm'd ſpirit of Wine upon very finely powder'd Amber, (which if it be the higher-colour'd yields the deeper Tincture) in a very gentle heat, (for the neglect of which Caution even expert Artiſts have often loſt their pains and glaſſes) we have ſeveral times had a good yellow Tincture of Amber, which was diſcernable in the Menſtruum both by the ſmell

in, well luted on, and beſides were in ſome places crackt, I ſuſpected that the Heat of the Dunghil had not only looſened the ſaline parts of the Liquor, but driven them away: and accordingly by diſtilling it in a very gentle heat, and in a very high Cucurbit, we obtain'd inſtead of an active and ſaline ſpirit, a languid and nauſeous phlegm. And how great odds there may be betwixt ſome Experiments made with recent and putrifi'd Urine, may be eaſily conceiv'd by him who knows what operations Salts have in the buſineſs of Colours, and is acquainted with their efficacy in thoſe other Mechanical Experiments wherein Urine is wont to be employ'd. But I fear I have dwelt too long upon this Theme, and therefore I ſhall proceed to the next.

And in the third place, *Pyrophilus*, I ſhall obſerve to you, that there is a great difference to be found among many things prepar'd by Art, that paſs under the ſame general name: which difference may proceed partly from that which we have already obſerv'd to be found in the Materials of which ſuch factitious Bodies are made, and partly from the way us'd in preparing them. To theſe heads many particulars may be reduc'd: But we ſhall at preſent reſtrain our ſelves to the mention of two ſorts of prepar'd bodies, namely, of ſuch as are not putrifi'd and exalted enough, and of ſuch as are ſo too much.

And to begin with the firſt of theſe; it is very certain, that divers Chymical Experiments delivered by ſober Authors have been believed falſe, only becauſe the Menſtruums or other Materials emyloy'd in the unſucceſsful tryals of them were not as highly rectifi'd, or otherwiſe as exquiſitely depurated, as thoſe that were us'd by the Deliverers of thoſe Experiments; ſo that oftentimes the fault of a bad Menſtruum is injuriouſly imputed to a good Artiſt. That experienc'd Chymiſt *Van Helmont*, in his Paradoxical Treatiſe of the Stone, endeavors (as we have elſe-

elſewhere mention'd) to explicate the manner of its being generated by the Coagulation immediately enſuing upon the mixture of the two volatile Spirits of Urine and of Wine. This noble Experiment has been by many unſucceſsfully try'd, and has been therefore by them diſcredited as a Chymical fiction: and indeed the firſt, and I think the ſecond time we attempted to make that Coagulum, we found nothing at all of any ſuch thing as we expected upon the confuſion of the two fore-mentioned Liquors, which though never ſo much ſhaken, and afterwards permitted to reſt, did never in the leaſt meaſure coagulate, which made us long ſuſpect the Experiment; till at length our favourable thoughts of that expert Chymiſt, making us think it poſſible that the Spirits we employ'd had not been ſufficiently exalted, we dephlegmated ſome by more frequent, and indeed tedious Rectifications (which yet prov'd but neceſſary) and then were ſatisfied by more accurate tryals, that *Helmont* had not miſ-inform'd us.

So likewiſe the ſame Author in his Treatiſe *de Peſte* much extolling, as a friend to the Stomach, the Entrails, the nervous parts, and even the Head, the Tincture or Solution of Amber made with ſpirit of Wine (which Medicine is indeed no ignoble one when adminiſtred to Conſtitutions that can well bear the heat of it) divers Phyſitians and Chymiſts have attempted the preparing of this Tincture with ſuch bad ſucceſs, that they have given out, that either *Helmont* delivered what was not true, or conceal'd ſome conſiderable Circumſtance of the Proceſs.

Whereas having digeſted ſufficiently dephlegm'd ſpirit of Wine upon very finely powder'd Amber, (which if it be the higher-colour'd yields the deeper Tincture) in a very gentle heat, (for the neglect of which Caution even expert Artiſts have often loſt their pains and glaſſes) we have ſeveral times had a good yellow Tincture of Amber, which was diſcernable in the Menſtruum both by the ſmell

and taſte; and to ſatisfie ſome that ſuſpected the Tincture to proceed but from the exaltation of the Menſtruum it ſelf by Digeſtion, and to manifeſt that it was a real Solution of the ſubtiler parts of the Amber, we poured ſome drops of it into Beer, or Water, into which the ſpirit of Wine ſuddenly diffuſing it ſelf, the diſſolved Amber was plainly diſcernable ſwimming like a thin film upon the ſurface of the Liquor, whence little by little it ſteamed away into the air.

There is likewiſe, as we have try'd, to be drawn with ſpirit of Wine from pure Salt of Tartar a pretty high Tincture, and of a taſte which I thought not unworthy the taking notice of: but having a while ſince try'd to draw this Tincture with ſpirit of Wine which (unknown to me) was much too weak for that purpoſe, after I had kept the Glaſs a while in Digeſtion, coming to look whether or no the Spirit was ting'd, I found that the Salt of Tartar had drawn to it ſelf and imbib'd the aqueous particles of the Spirit of Wine, and being thereby (for a great part of it) diſſolv'd into a Liquor like that which is commonly called *Oyl of Tartar per Deliquium*, the ſubſiding Salt was by the interpoſition of that ſaline Liquor protected from the action of the ſpirit of Wine, which being by this new way dephlegm'd would not mix with the ſaline Liquor, but ſwam entirely above it. To which I ſhall only add in general, that the German Chymiſts are divers of them ſo accurate in the Rectification of their Spirit of Wine, that in *England*, where we are wont to be leſs careful about that Particular, it is uſual enough for thoſe Experiments of theirs to be unſucceſsfully try'd wherein the Alcohol of Wine (as they call it) is requiſite.

And as Spirit of Wine, ſo many other Menſtruums are made unfit for the perfecting of divers real Experiments, barely by their not being ſufficiently freed from their weakning Aquoſity.

Nor

Nor is it only, *Pyrophilus*, in Menſtruums, but in divers other Bodies, that the want of an exquiſite Depuration may produce in Experiments variety of Events. As for inſtance, It has been complain'd of by ſober men, that their Preparations of Silver, though never ſo carefully made, have been apt to produce violent Vomits; whereas we have not obſerv'd a well-prepar'd Medicine of duly refin'd Silver to work Emetically even in Women and Girls, but by Seige or Urine. But we cannot wonder at the violent operation of Medicines made of ordinary Silver: for not only that which is coyned is wont, as the Mint-maſters themſelves have confeſs'd to me, to be allay'd with ſometimes about a twelfth part, ſometimes a ſmaller or greater proportion of Copper, for the greater conveniency of the Coyn, but even that Silver which is commonly at great rates ſold for refin'd Silver, is not wont to be ſufficiently freed from its Copper. Which I not long ſince manifeſted in the preſence of one of our richeſt and eminenteſt Refiners, by diſſolving ſome of his pureſt Silver in his own *Aqua fortis*; for the greenneſs of the Solution quickly betray'd the adherency of *Venus* to the Silver. And no wonder; for I have ſeldom ſeen our chiefeſt Refiners blow off from their Silver upon the Teſt above half its weight of Lead, whereas we think not our Silver ſufficiently refin'd for ſome purpoſes, till it have been freed from five or ſix times its weight of *Saturn*, and then it has ſometimes afforded a Solution almoſt as clear as water, with only now and then a light touch of Sky-colour, but nothing near ſo high as the Ceruleous (Liquor that is ſuppoſed to be a true) Tincture of Silver, artificially ſeparated from the reſt of the Body.

Now that ill effects by the mixture of Copper may be produc'd in ſuch Medicines as ought to be of pure Silver, he that is acquainted with the violent Emetick qualities of *Venus* can ſcarcely doubt. And as in men's bodies, ſo in other

ſubjects, thoſe Experiments may eaſily deceive the Artiſts expectation, when he hopes to perform with Silver and Copper together thoſe things which ſuppoſe and require Silver without Copper, or any adventitious Metal. And as Silver, ſo Gold is very often employ'd for pure, when it is not ſo: for even the foliated Gold which is commonly ſold here in *England*, how fine ſoever 'tis reputed, is not altogether free from the pollutions of other Metals: for our Gold-beaters, though for their own profit ſake they are wont to uſe the fineſt coyned Gold they can get (as that which is capable of the greateſt extenſion under the Hammer) yet they ſcruple not to employ coyned Gold, and that the Mint-maſters (as themſelves inform me) are wont to allay with Copper or Silver, to make the Coyn more ſtiff, and leſs ſubject to be waſted by attrition. And as for thoſe many Gold-ſmiths and Chymiſts who think their Gold moſt requiſitely refined when they have blown from it on the Teſt a due proportion of Lead, they may be therein ſometimes miſtaken: for though *Saturn* may carry away with him all the Copper that did imbaſe the Gold, yet he does not likewiſe free it from the Silver (for which purpoſe *Aqua fortis* is therefore wont to be us'd) nay, the skilfulleſt Refiner that I ever yet knew, hath ſeveral times affirmed to me, that coupleing fine Gold with Lead, the Gold has after retained and protected from the fire a proportion of Silver that lay lurking in the Lead, and was afterwards ſeparated from the Gold by *Aqua fortis*, but in ſo ſmall quantity, that the Experiment (the coſt and pains conſidered) was not lucriferous. And of this ſort of Inſtances, *Pyrophilus*, more might be preſented, if we did not think Prolixity might be unwelcome to you.

But as many Experiments ſucceed not according to expectation, becauſe the Menſtruums employ'd about them were not pure enough, ſo ſome miſcarry becauſe ſuch Menſtruums

ſtruums are but too exactly depurated: for it is not ſo much the purity of Liquors in their kind, as their fitneſs for the particular purpoſe to which they are deſign'd, that is in Experiments to be principally regarded. For inſtance, we have ſometimes for recreation ſake, and to affright and amaze Ladies, made pieces of white paper and linnen appear all on a flame, without either burning, findging, or as much as diſcolouring them. This is performed by plunging the paper very throughly in weak Spirit of Wine, and then approaching it to the flame of a candle, by which the ſpiritous parts of the Liquor will be fired, and burn a pretty while without harming the paper. But if this Experiment be tryed with exquiſitely rectifi'd Spirit of Wine, it will not ſucceed. Of this Phænomenon this plauſible reaſon has been aſſign'd, that the flame of the Spirit of Wine is ſo pure and ſubtile, that like an *Ignis lambens*, it will not faſten upon the paper. But Experience has inform'd us, that this Conjecture is but a miſtake, for the flame of Spirit of Wine is ſo hot, that I have in Lamp-furnaces employ'd Spirit of Wine inſtead of Oyl, and with the ſame flame I have not only lighted paper, but candles, and even melted foliated gold. The true reaſon therefore why that paper is not burned by the flame that plays about it, ſeems to be, that the aqueous part of the ſpirit of wine being imbibed by the paper keeps it ſo moiſt, that the flame of the ſulphureous parts of the ſame ſpirit cannot faſten on it. And therefore when the deflagration is over, you ſhall always find the paper moiſt; and ſometimes we have found it ſo moiſt, that the flame of a candle would not readily light it. And on the other ſide, having purpoſely made tryals of plunging paper into ſufficiently dephlegmated ſpirit of wine, the paper not having aqueous moiſture to defend it, was very readily kindled and burned by the flaming ſpirit. And one of our beſt ways to try the pureneſs of ſpirit of wine is grounded on this

very

very ſuppoſition: For dipping it in a Cotton-wiek like that of a candle, and ſetting it on fire, if the flame faſten on the wiek, it is a ſign of the goodneſs of the ſpirit; but if it do not, we conclude it to be weak, and not ſufficiently dephlegm'd. It hath been likewiſe obſerv'd, that *Aqua fortis* will work more readily on Lead if it be allay'd with water, than if it be purely rectifi'd. I other-where alſo mention an *Aqua fortis* I have us'd, which was ſo ſtrong, that it would not well diſſolve ſilver it ſelf unleſs I firſt diluted it with fair water. And within this very week wherein I write theſe things, I have had an unwelcome proof that Liquors may by too exquiſite a Depuration be made unfit for our purpoſes. For having, to gratifie ſome ingenious friends, made a certain Menſtruum, wherewith we had formerly done ſome things upon Gold which were (not altogether without cauſe) thought ſtrange enough, we took care at this time to ſeparate it from whatever was either of an aqueous or an earthy nature more exactly than ever we had formerly done. But coming to make uſe of this ſort of Menſtruum, we found to our trouble and loſs, that inſtead of performing its wonted operations upon Gold better than before, we could do nothing at all with it: For it will not now by heat it ſelf be brought to touch gold, though that Metal were wont to be diſſoluble in it even in *frigido*. And to ſatisfie you, that 'twas the too-exquiſite depuration of this Liquor, eſpecially from its terreſtrial parts, that thus unfitted it to touch a Metal which is otherwiſe wont to melt as it were naturally in it without Ebullition (almoſt like Ice in luke-warm water) we will ſubjoyn, that not only we in vain try'd to make it ſerviceable by weakning it with fair water; but having, for tryal ſake, taken a little of this numerical parcel of Liquor before it was ſo carefully rectifi'd, we found that it diſſolv'd crude gold as well as we had reaſon to expect. And it would be conſider'd whether or no in the Extracti-

on

on of the Tinctures of ſeveral Bodies, Chymiſts do not only put themſelves to a needleſs, but to a prejudicial trouble, when they refuſe to employ any other ſpirit of wine than that which is highly rectifi'd. For, though in many Bodies the parts deſir'd by the Artiſts being the Sulphureous ones, the Menſtruum is the better for an exquiſite Dephlegmation; yet in divers other Concretes the uſeful and efficacious parts have in them ſomething of Saline, which makes them more free to impregnate copiouſly ſuch Liquors as have ſome Aqueous mixed with their Sulphureous parts.

But becauſe there is nothing more eaſie than by diluting ſpirit of wine, though never ſo ſtrong, to make it as weak as one pleaſes; and becauſe pure ſpirit of wine is that of all other Menſtruums that Chymiſts generally make moſt uſe of, and which coſts them moſt of charge and trouble, (inſomuch that here in *London* that which is perfectly dephlegm'd is valu'd, in their ſhops that ſell both, at ten times the price of common ſpirit of wine;) I preſume you will not take it ill, that without being oblig'd to it by the Title of this Diſcourſe, I take this occaſion to acquaint you with the way I employ to obtain dephlegm'd ſpirit of wine: Eſpecially ſince the practice of the common way of frequent Rectifications is (not to mention other Inconveniences) wont to prove either exceeding tedious, or inſufficient. Put then about an inch thick of Tartar calcin'd to whiteneſs (for I find it not neceſſary to reduce it to a Salt) and very dry into the bottom of a tall and ſlender Glaſs body, and pour on it as much ſpirit of wine that has been but once rectifi'd, as will, when they have been ſhak'd together, ſwim above the Tartar a fingers breadth (more or leſs in proportion to the Tartar you put in) and then the Head and Receiver being carefully faſten'd on again, in a gentle heat draw off the ſpirit of wine, ſhifting if you pleaſe the Receiver when about half is come over, and if need

need be, rectifying once more all that you diſtil upon dry Calx of Tartar as before. Whether or no you may meet with this Method in ſome Chymical Books, I know not: But it ſeems that either it has not been clearly taught, or has been propos'd by ſuſpected Authors, or elſe among other Proceſſes, by being found in whoſe company it has been diſcredited. For the moſt ancient and experienc'd Diſtillers I have met with, have either contented themſelves to follow the common way of repeated Rectifications, though thereby they loſe much time, and much ſpirit of wine; or elſe have had recourſe to peculiar Veſſels of ſuch a height, as beſides that they are neither eaſily nor cheaply to be procur'd, do not, as far as I have hitherto ſeen, excuſe the need of reiterated Rectifications. Whereas, when we conſider'd that the fix'd Salt of Tartar readily imbibes Aqueous bodies, and that yet it will not at all mix with pure ſpirit of wine, it was eaſie to conclude, that the Phlegmatick part of the ſpirit of wine would be ſoak'd up by the Alcalizate Salt, whereby the inflammable part would be freed from it. And accordingly when we proceeded after the manner above preſcrib'd, we found that the Liquor that was produc'd upon the firſt Rectification from the Salt, being fir'd in a warm Silver-ſpoon, did not leave behind it one drop of Phlegm, or ſo much as the leaſt moiſture upon the ſpoon, nay, and indeed did indure a ſeverer Examen, to which for curioſitie's ſake we thought fit to put it. And when the Diſtillation was carefully made, we found by frequently (for tryal ſake) ſhifting the Receiver, that all the Spirit that aſcended was (to ſenſe) equally pure, ſince that which came up laſt of all, even till the Calx ſeem'd to begin to grow dry, by beginning to cleave at the top, did burn all away, as well as that which came over firſt. And having for further tryal taken out the calcin'd Tartar, and diſtill'd it with a good fire, it yielded us pretty ſtore of a nauſeous and ſtrongly-ſcented Liquor,

Liquor, which ſeem'd to be but Phlegm, both to the taſte, and by its not being at all inflammable though carefully try'd. The ſame Calx of Tartar being kept in ſome earthen Veſſel upon the fire till it be well dry'd, which will require a good heat, may be employ'd more than once in this operation. And 'twas not needleſly that we preſcrib'd Bodies tall and ſlender: For we found not the Experiment to ſucceed in large and low ones, and much leſs in Retorts, in which the Phlegm is wont to riſe together with the Spirit; yet we found, that provided the diſtillation were made with a ſufficiently mild heat, a Glaſs, though very broad, and but moderately high, would ſerve the turn ſo far, as that the firſt half that aſcended (the other being very weak) prov'd a Spirit that in a ſilver-ſpoon would burn perfectly all away. And becauſe white Calx of Tartar is ſometimes not ſo eaſie to be procur'd, we will add, that we have for tryal ſake ſometimes ſubſtituted Quick-lime, or Salt of Pot-aſhes, (made by a ſingle Solution, Filtration, and Coagulation) with no bad ſucceſs, eſpecially in caſe of removing the Receiver before the Aſcenſion of the laſt part of the Liquor, though even that it ſelf has ſometimes from Quick-lime come up inflammable enough. And therefore this Alcohol of wine we peculiarly call the Alcalizate Spirit of Wine; and the rather, becauſe *Spiritus vini Tartarizatus*, which perhaps may be thought the propereſt name for it, is employed by eminent Chymical Writers to ſignifie a different thing. And a practicable way of making ſuch an Alcaliz'd and pure Spirit of Wine, we thought not unfit to teach you here once for all, in regard the Menſtruum is ſo highly uſeful, not only for Tinctures, Extracts, and many other Chymical operations, but in the making of divers Philoſophical Experiments, and particularly ſome of thoſe which you may meet with in our Writings. And an eminently ingenious perſon (but to me a ſtranger) chancing to get a ſight of this Eſſay, was pleaſed

to give me thanks for this laſt part of it, becauſe, though he had very often made uſe of Salt of Tartar to improve Spirit of Wine; yet he did it before, not to dephlegm the weaker Liquor, but to acuate the ſtrong with the Alcali: Which though I deny not to be a thing feaſible, yet (as I told him) unleſs it be skilfully attempted, the highly rectifi'd Liquor that is poured on, will rather leave ſome of its moſt ſpiritous parts behind, than carry up ſo fixt a Salt.

THE

THE SECOND ESSAY,

Of Un-ſucceeding EXPERIMENTS.

WHAT has been already ſaid, *Pyrophilus*, may, I hope, ſuffice to ſhew you, how Experiments may miſcarry upon the account of the Materials employ'd in trying them. And therefore we ſhall now paſs on to conſider the Contingencies to which Experiments are obnoxious upon the account of Circumſtances, which either are conſtantly unobvious, or at leaſt are ſcarce diſcernable till the Tryal be paſt. And becauſe theſe Circumſtances can hardly be diſcours'd of in an accurate Method, (which their nature will ſcarce admit of) I ſhall not tye my ſelf to any other order in ſetting down the Inſtances which occur to me on this occaſion, than that wherein they offer themſelves to my memory.

And firſt I muſt acquaint you with what was not long ſince ſeriouſly related to me by Doctor *K.* a perſon exceeding far both from the Cuſtom, and in this particular from the Temptation of telling untruths. He then aſſur'd me,

that lending his Laboratory in *Holland* to a friend of his during his own abſence, and leaving in that Laboratory among other things great ſtore of *Aqua fortis* of ſeveral compoſitions which he had made, to employ about his famous Scarlet Dye, this friend of his ſent him word a while after his departure, that by digeſting Gold with an *Aqua fortis*, he had ſeparated the Tincture or yellow Sulphur from it, and made it volatile, (the remaining body growing white) and that with this golden Tincture he had, not without gain, turn'd Silver (as to part of it) into very perfect Gold. Upon which advertiſement the Doctor ſpeedily returning to his Laboratory, did himſelf with the ſame *Aqua fortis* divers times draw a volatile Tincture of Gold, which did turn Silver into true Gold: and (that I may add That upon the by, to gratifie your curioſity) when I demanded whether or no the Tincture was capable to tranſmute or graduate as much Silver as equall'd in weight that Gold from whence the Tincture was drawn, he aſſur'd me, that out of an ounce of Gold he drew as much Sulphur or Tincture as ſufficed to turn an ounce and a half of Silver into that nobleſt Metal. Which I am the more diſpoſed to believe, partly becauſe my Tryals permit me not to doubt of the ſeparableneſs of a yellow Subſtance or Tincture from Gold, and partly becauſe I am tempted to think, that Silver may have in it a Sulphur (to ſpeak in the Chymiſts Language) which Maturation is capable to graduate into a Golden one, by having been certifi'd by the obſervations of men very experienced in Metalline Affairs, (and perhaps too by my own) that ſometimes by corroſive Liquors (which Sir *Francis Bacon* alſo, if I miſtake not, ſomewhere obſerves) and ſometimes by the operation of common Sulphur (eſpecially well open'd and aſſociated with fit Salts) Silver has afforded ſome grains of very pure Gold. But our Doctor found himſelf much miſtaken in the hopes of growing rich by this Experiment: For a while

while after endeavouring to make it again, his hopes were fruſtrated, which he aſcribes to the *Aqua fortis*, and therefore has attempted the ſame work afreſh. But ſince all his Tryals have been hitherto fruitleſs, 'tis not improbable that the diſappointment proceeded from ſome other more abſtruſe cauſe; for we find ſuch Adventures to have ſometimes befallen Artiſts irreparably. And *Glauber* alone, if you will therein credit him, tells us of ſeveral ways by which he made Gold once, and could not do it again. Upon which Subject I muſt not omit thoſe very illuſtrious Teſtimonies and Inſtances of this nature, that I find recorded by that Ornament of his Age and Quality, the Prince of *Mirandula*, in his Treatiſe *de Auro*.

Novi (ſays he) *qui mihi aſſeruerit ſemel ſe ex mobili argento quod vivum dicitur ſtabile verumque argentum confeciſſe ſuccis & foliis herbarum, idque vendidiſſe peritis explorandæ Metallicæ veritatis; eiſdem mox uſum ſe foliis fruſtra, & quod ſemel perfecerat, nunquam alias, quanquam id ſæpe tentaverit, perficere potuiſſe.* Lib. 3. cap. 6.

Alium novi (ſays he further) *qui adhuc apud vivos moratur, cui cum aurum & argentum circiter Quindecies per artem effectum eſſet, amiſit artem eam, accepitque oraculo ſocii per quietem habito, id ingratæ mentis vitio contigiſſe. Ut hinc etiam veritatem Apoſtolici dicti condiſcamus, Neque qui plantat, neque qui rigat eſt aliquid, ſed incrementum dat Deus.* And to both theſe Narratives our learned Prince does in the ſame Book add divers others. *Retulit quidam mihi* (ſubjoyns he) *ſeſe Aurum ex argento feciſſe ſemel magnâ copiâ; ſecundo ſe uſum eiſdem rebus, feciſſe quidem, ſed minimâ ſemper Quantitate, ſic ut detrimentum lucro majus eſſe ſupputaverit. Veniſſe in mentem uti detrimentum effugere poſſit, ſi non ex argento, ſed ex ære melioris conditione metalli, ſeſe conſequi experiretur, idque ſe conjecturis firmis nixum tentaviſſe: cumque in eo fuiſſet ut rem ſeſe adepturum ſperaret, miris modis eveniſſe, ut nihil omnino conſequeretur.*

Idem

Idem (continues the Prince) *affirmavit ab amico qui experto hoc ipſum fuerat accepiſſe, qui cum ex Cinnabari argentum feciſſet optimum, ſæpenumero ſeſe poſtea inſiſtentem operi majore cum diligentia ſemper eventu rei fuiſſe fruſtratum.* And to theſe Relations of this famous Prince I could add others of ſome Acquaintances of mine, who having either once or twice made *Luna fixa* (as Artiſts call that Silver, which wanting but the tincture of Gold abides the tryal of *Aqua fortis*, &c.) or ſome other Lucriferous Experiment, have ſince in vain attempted to do the like again, and yet have their eyes ſo dazel'd by the Gold and Silver they have (either ſeparated or) made, that they are not to be prevailed with to deſiſt from proſecuting their uncertain hopes.

That diverſe Experiments ſucceed when try'd in ſmall quantities of matter which hold not in the great, it may ſave you ſomething to be advertiſed of; diverſe Projectors, eſpecially Chymiſts, having already very dearly bought the knowledge of that truth. For oftentimes a greater and unwieldy Quantity of matter cannot be expoſed in all its parts to a juſt degree of fire, or otherwiſe ſo well manag'd as a leſs Quantity of matter may be order'd. But this is ſo manifeſt a truth to thoſe that have dealt much in Experiments, that whereas many Chymiſts would be vaſtly rich, if they could ſtill do in great Quantities what they have ſometimes done in little ones, many have undone themſelves by obſtinately attempting to make even real Experiments more gainful.

I have not been very ſollicitous to ſubjoyn Particulars to the foregoing Obſervations, becauſe that by reaſon of the Contingency of ſuch Experiments as would be the moſt for my preſent purpoſe, you might poſſibly be tempted to loſe toyl and charges upon tryals very likely not only to delude your hopes, but perhaps to make you diſtruſt the fidelity of our relations. Yet for Illuſtration ſake of what we have delivered, I am willing to mention

ſome

ſome few contingent Experiments that occur to my thoughts.

And firſt, it is delivered by the Lord *Verulam* himſelf, as I remember, and other Naturaliſts, that if a Roſe-buſh be carefully cut as ſoon as it has done bearing, it will again bear Roſes in the Autumn. Of this many have made unſucceſsful tryals, and thereupon report the Affirmation to be falſe; and yet I am very apt to think, that the Lord *Verulam* was embolden'd by Experience to write as he did. To clear up which difficulty, let me tell you, that having been particularly ſollicitous about the Experiment, I find by the relation both of my own and other experienc'd Gardeners, that this way of procuring Autumnal Roſes will in moſt Roſe-buſhes moſt commonly fail, but in ſome that are good bearers it will ſucceed; and accordingly having this Summer made tryal of it, I find, that of many buſhes that were cut in *June* in the ſame Row, the greater number by far promiſe no Autumnal Roſes, but one that hath manifeſted it ſelf to be of a vigorous and prolifick nature, is at this preſent indifferently well ſtor'd with Damask-roſes. And there may be alſo a miſtake in the kind of Roſes: for experienc'd Gardeners inform me, that the Musk-roſe will, if it be a luſty Plant, bear flowers in Autumn without the help of cutting. And therefore that may be miſaſcrib'd to Art, which is the bare production of Nature. And Cinnamon Roſe-buſhes do ſo much better thrive by cutting than ſeveral other ſorts, that I remember, this laſt Spring my Gardener having (as he told me) about mid-*April* (which was as ſoon as that kind of Roſe-buſh had done bearing) cut many of them in my Garden, I ſaw about the middle of *June* ſtore of the ſame buſhes plentifully adorn'd both with Buds and with blown Flowers.

An uncertainty not unlike that which we have newly taken notice of in the Experiment of producing Autumnal Roſes, has been likewiſe obſerved in the attempts that have

have been made to make diverſe ſorts of Fruit grow upon the ſame Tree. And as for differing ſorts of Fruits of the ſame denomination, as Apples, Pears, &c. though ſome ſevere Naturaliſts are unwilling to believe that they can be made to grow upon the ſame Tree; yet we dare not imitate their ſeverity, having lately ſeen various ſorts of Pears fed by the ſame Tree, and elſewhere three and twenty ſorts of Apple-Grafts flouriſhing upon the ſame old Plant, and moſt of them adorn'd with Fruit. Nay, and though the Fruits be not of the ſame denomination, yet if they be of kin in nature, they may very poſſibly be brought to grow on the ſame Tree: for we lately gathered ripe Apricocks and ripe Plums upon one Tree, from which we likewiſe expect ſome other ſorts of ſtone-fruit. But to make fruits of very differing natures be nouriſhed proſperouſly by the ſame ſtock, is ſo difficult a thing, that we can at moſt but reckon it among contingent Experiments: for though *Pliny* and *Baptiſta Porta* relate their having ſeen each of them an example of the poſſibility of producing on one Tree great variety of differing fruits; and though ſuch a perſon as the deſervedly-famous Aſtronomer Dr. *Ward* aſſures me, that he has particularly taken notice of Pears growing upon an Apple-tree; and I elſewhere add a reſembling Obſervation of ours; yet certainly this Experiment has been for the moſt part but very improſperouſly attempted, nor have I yet ever ſeen it ſucceed above once, though try'd with very much care and induſtry. And I remember that this very year, in the ſame Garden where I gather'd the Apricocks and Plums above mention'd, I ſaw the Ciens of a Pear-tree ſo skilfully grafted upon an Apple-ſtock, that it flouriſh'd very much with bloſſoms in the Spring, and gave me great hopes that it would bear fruit this newly-paſt Summer, but has deceived my expectation, as divers other Plants ſo grafted in the ſame Garden have for many years deluded the hopes of

the

the skilful Maſter of it, who aſſures me, that though divers of them did for ſome years ſucceſſively afford promiſing bloſſoms, yet they all decay'd away without bearing any of them any fruit. Which yet may ſeem ſomewhat ſtrange, ſince not only we have this Summer gather'd Pears upon a graft which a Divine, to whom the Garden belongs, affirmed to have been grafted upon a Quince-tree; and the induſtrious *Kircher* tells us, that *Experientia docet Perſicum Moro inſitum fructus proferre, &c. de quo nullum eſt dubium utpote vulgare penè*: but experience tells us, that as little as a White-thorn and a Pear-tree ſeem of kin, a Ciens of the latter will ſometimes proſper well being grafted upon a ſtock of the former. *Artis Mag. Lucis & Umbræ lib. 1. p. 3. cap. 6.*

To contingent Experiments, *Pyrophilus*, you may if you pleaſe refer what is delivered by thoſe learned Writers, who affirm, That if a Lixivium made of the Aſhes or fixt Salt of a burn'd Plant be frozen, there will appear in the Ice the Idea of the ſame Plant: For we have divers times purpoſely made trial of this Experiment without the promiſed ſucceſs: and I remember that in the laſt cold ſeaſon, proper for ſuch trials, I purpoſely made a Lixivium of fair Water and Salt of Wormwood, and having frozen it with Snow and Salt after the manner of Congelation elſe-where declar'd, I could not diſcern in the Ice any thing more like to Wormwood than to ſeveral other Plants; and having about the ſame time, and after the ſame manner, expos'd to congelation a thin Vial full of a ſtrong Decoction of Wormwood, (from which an Idea of the Plant may be more probably expected) thoſe to whom I ſhew'd it after it was frozen could diſcern as little like Wormwood in it as my ſelf. 'Tis true, that in both theſe Vials the Ice ſeem'd ſomewhat odly figur'd; but it is true alſo not only that we have obſerved that Water wherein a ſaline body, as Salt-peter, or Sea-ſalt, or Sugar, *&c.* has been diſ-

ſolv'd, has afforded us Ice which ſeem'd to ſhoot into ſeveral figures, but even in ordinary water congealed we have often ſeen Ice figur'd, as if the water had been no Elementary body; which needs not be admir'd, ſince (to omit other cauſes which may concur to the production of this effect) many Waters gliding through Earths abounding in ſaline particles of this or that nature, may be eaſily, in their paſſage, impregnated with them; whence perhaps it comes to paſs, that Dyers find ſome Waters very fit, and others very unfit for the dying of Scarlet and ſome other Colours. And therefore we cannot but think that the figures that are oftentimes to be met with in the frozen Lixivium or Decoction of a Plant, will afford but uncertain proofs that the Idea of each, or ſo much as of any determinate Plant, diſplays it ſelf conſtantly in that frozen Liquor. And I much fear, that moſt of thoſe that tell us that they have ſeen ſuch Plants in Ice, have in that diſcovery made as well uſe of their Imagination as of their Eyes. And 'tis ſtrange to obſerve what things ſome men will fancy, rather than be thought to diſcern leſs than other men pretend to ſee. As I remember Mr. *R.* the juſtly famous maker of Dioptrical Glaſſes, for merriment telling one that came to look upon a great Tube of his of 30 foot long, that he ſaw through it in a Mill ſix miles off a great Spider in the midſt of her Web; the credulous man, though at firſt he ſaid he diſcern'd no ſuch thing, at length confeſſed he ſaw it very plainly, and wonder'd he had diſcover'd her no ſooner. But yet, *Pyrophilus*, becauſe two or three ſober Writers do ſeriouſly relate ſome ſtories of that nature upon their own obſervation, I am content for their ſakes to reckon their Experiments rather among the Contingent than the abſolutely falſe ones: for it is not impoſſible but that among the many figures which frozen Liquors do ſometimes put on, there may appear ſomething ſo like this or that Plant, that being look'd upon with the favourable

favourable eye of a prepoſſeſs'd beholder, it may ſeem to exhibit the Picture of the calcin'd Vegetable: and we our ſelves not very long ſince, ſetting to freeze in Snow and Salt a fine green Solution of good Verdegreaſe, (which contains much of the Saline parts of the Grapes coagulated upon the Copper by them corroded) obtain'd an Ice of the ſame colour, wherein appear'd divers little figures, which were indeed ſo like to Vines, that we were ſomewhat ſurpriz'd at the Experiment; and that which encreas'd our wonder was, that another part of the ſame Solution being frozen in another Vial by the bare cold of the air, afforded us an Ice angularly figur'd, (as we have obſerv'd the Ice of ſaline Liquors oftentimes to be) but not at all like that made by the application of Snow and Salt. And having for further trial ſake ſuffered that Ice wherein the Vines appear'd to thaw of it ſelf, and having then frozen the Liquor a ſecond time in the ſame Vial, and after the ſame manner as formerly, we could not diſcern in the ſecond Ice any thing like that which we had admir'd in the firſt. And in Wine and Vinegar, as much as thoſe Liquors partake of the nature of the Vine, we have not after Congelation obſerved any peculiar reſemblance of it in figure.

The mention we have been making of Ice brings into my memory another Experiment, which may perhaps be reckon'd likewiſe among Contingent ones, and that is the Experiment of burning with Ice as with a Glaſs Lens; which though ſome eminent Modern Writers preſcribe to be done without taking notice of any difficulty in it, yet both we and others that have induſtriouſly enough try'd it, have met with ſuch defeating circumſtances in it, eſpecially from the ununiform Texture wont to be met with in moſt Ice, that the making of ſuch burning-Glaſſes may be well enough referr'd to thoſe Experiments whoſe conſtant ſucceſs is not to be rely'd on, as we elſe-where more particularly declare.

In the Trade of Dying there is ſcarce any tinging Ingredient that is of ſo great and general uſe amongſt us as Woad or Glaſtum; for though of it ſelf it Dye but a Blew, yet it is us'd to prepare the cloath for Green and many other of the ſadder Colours, when the Dyers have a mind to make them permanent and laſt without fading: but yet in the decocting of Woad to make it yield or ſtrike its colour, there are ſome critical times and other circumſtances to be obſerved; the eaſie miſtake of which oftentimes defeats the Dyers expectation to his very great loſs, which ſometimes he knows not to what to impute, of which I have heard ſeveral of them complain. And therefore divers of our leſs-expert Dyers, to avoid thoſe hazards, leave off the uſe of Woad, though growing plentifully enough here in *England*, and inſtead of it employ Indico, though it coſt them dearer, as being brought hither ſometimes from *Spain*, ſometimes from the *Barbadoes*, and oftentimes even from the *Eaſt-Indies*.

Our *London* Refiners, when to part Silver and Copper they diſſolve thoſe mixed Metals in *Aqua fortis*, are wont afterwards to dilute the glutted Menſtruum with ſtore of fair water, and then with Copper Plates to ſtrike down the diſſolv'd Silver. But becauſe by this manner of proceeding much Copper is wont after the ſeparation of the Silver to remain in the Menſtruum, as may appear by its high tincture, that this thus impregnated Liquor may be improv'd to the beſt advantage, they are wont to pour it upon what they call Whiting (which is ſaid to be a white Chalk or Clay finely powder'd, cleans'd, and made up into Balls) wherewith the tincted parts incorporating themſelves, will in ſome hours conſtitute one ſort of Verditer fit for the uſe of Painters, and ſuch other Artificers as deal in Colours, leaving the remaining part of the Menſtruum an indifferently-clear Liquor, whence they afterward by boiling reduce a kind of Salt-peter fit with the addition of Vitriol

(and

(and ſome freſh Niter) to yield them a new *Aqua fortis*.

And theſe things I mention, *Pyrophilus*, that you may know what I mean when I tell you, that ſometimes the Refiners cannot make this Verditer for a great while together, and yet cannot tell whence their diſability to make it proceeds. Of which Contingency I remember I lately heard one of the eminenteſt and richeſt of them ſadly complain, affirming, that neither he, nor divers others of his Profeſſion, were able, not long ſince, to make Verditer for divers months together; and that ſeveral others were yet at a loſs in reference to that particular: though for his part he had, without knowing the Cauſe of this Contingency, found a Remedy for it, namely, to warm the Menſtruum well before it be poured on the Whiting, on which, when the Liquor was warm, the tincted parts would faſten, though they would not, whilſt (according to the cuſtom of Refiners) it was poured on cold.

Making likewiſe the other day a viſit to the chief Copperas work we have in *England*, one of the Overſeers of it, who went along with me to ſhew me the contrivance of it, aſſured me, that divers times, by the miſtake or neglect of a circumſtance in point of time, they had loſt, and are yet ſubject to loſe, ſome thouſands of pounds of Vitriol at a time, which in ſpight of their wonted, but not ſufficiently-attentive and skilful care, would degenerate into an Unctuous Subſtance, not to be reduc'd into good Vitriol again; unleſs by the tedious way of throwing it abroad, and expoſing it with the unprepared ſtones, from which they draw their Vitriol, to the Rain and Sun to be open'd anew, and fitted for the yielding of Vitriol after the ſame manner with thoſe crude Minerals.

Upon this occaſion I muſt not omit, becauſe much conducing to the ſcope of our preſent Diſcourſe, a memorable Relation that I have met with in the Indian Hiſtory of the learned *Joſephus Acoſta*, who diligently ſurvey'd the famous

and

and almoſt ineſtimable Mines of *Peru*, and (for one that was not a Chymiſt) has delivered divers conſiderable and judicious Obſervations about them. That which I am now to mention is in that Chapter where he treats of the Silver of the *Indies*, ſet down in theſe words: It is ſtrange to ſee not only the difference betwixt the refining of Metal by Fire, and without it by Quick-ſilver, but alſo that ſome of theſe Metals which are refined by the fire, cannot well be molten with any Artificial Wind, as with Bellows, but when it is kindled and blown with the Natural Air or Natural Wind. The Metal of the Mines of *Porco* is eaſily refined with Bellows; and that of the Mines of *Potozi* cannot be molten with Bellows, but only by the breath of their Guayars, which are ſmall furnaces upon the ſides of the Mountains, built expreſly where the Wind lies, within the which they melt this Metal: and though it be hard to yield a reaſon of this difference, yet it is moſt certain and approv'd by long Experience.

Joſephus Acoſta lib. 4. cap. 5.

If there be any Trade that obliges the Artificers to be aſſiduouſly converſant with the Materials they employ, it is that of the Glaſs-men; and yet even to them, and in their moſt ordinary operations, there happen now and then little accidents, which though they know not well to what to aſcribe, are yet capable of hindring them from doing ſometimes what they have done a thouſand times. And I remember that among the laſt times that I have been at a Glaſs-houſe, an eminently-skilful Workman, whom I had purpoſely engag'd to make ſome Veſſels for me that required more than ordinary dexterity, was not able when I came thither to make Metal (as they call that colliquated mixture of Sand and fixt Salt whereof they blow their Glaſſes) tolerably fit to be employ'd: Wherefore he deſired me to take the pains to come again another day, and he would try to repair his unluckineſs. But the next time

time I came, though it were upon appointment, his Metal prov'd again unſerviceable, and inſtead of being colourleſs when it was cold, look'd as if it had been ſtain'd with Blew and Yellow, and was beſides britler than it ought to have been. So that it need be no ſuch wonder, if Philoſophers and Chymiſts do ſometimes miſs of the expected Event of an Experiment but once, or at leaſt but ſeldom try'd, ſince we ſee Tradeſmen themſelves cannot do *always*, what, if they were not able to do *ordinarily*, they could not earn their bread.

It is affirmed by *Helmont* and others that treat of the *Lapides Cancrorum*, that they grow within the skulls of thoſe Craw-fiſhes from whence they have their name: but I have known good Anatomiſts complain, that they have ſought them in vain in the heads of thoſe fiſhes, which may well make them diſtruſt the veracity of thoſe that aſcribe them to that ſort of Animals; yet we have often taken thoſe ſtony Concretions out of the heads of Craw-fiſhes. But paſſing lately through *Hungerford*, a Town famous for the plenty of ſuch kind of fiſh, we made diligent enquiry concerning their Nature, and were there informed by thoſe that looked to them, that the Concretions above mention'd are to be found in their heads but about that ſeaſon of the year wherein they ſhift their ſhells, and that at other times of the year, ſeveral perſons had in vain endeavoured to ſtore themſelves with Crabs eyes at *Hungerford*. And indeed, having at the laſt time of my being there (which was about the latter end of *June*) caus'd divers large ones to be taken out of the water, we found theſe little ſtones in the head but of one of them; whereas about a fortnight before, which was near the Summer Solſtice, paſſing by that place, we found in the wonted parts of the heads ſeveral ſuch Concretions as to bigneſs and ſhape, but ſo ſoft, that we could eaſily cruſh and diſcind them betwixt our fingers. And certainly, the miſtake

ſtake of the circumſtance of time has much prejudic'd the reputation of many truths: and I remember that *Aſellius*, to whoſe Anatomical fortune the world is ſo much beholden, ingenuouſly acknowledges, that he had like to have loſt the diſcovery of the milky veins, becauſe having at firſt ſuſpected thoſe unlooked for white Veſſels, which he took notice of in the Meſentery of a Dog diſſected alive, to be ſome irregular ramifications of Nerves, he was much confirmed in his conjecture by the next Dog he open'd; for having diſſected him at an inconvenient diſtance of time from the Dogs repaſt, the ſlender Veſſels he looked for being deſtitute of the Chyle, which is it that makes them conſpicuous, did not appear: So that he had loſt the benefit of his firſt lucky obſervation, had not his Sagacity inclin'd him to ſuſpect, that if a Dog were plentifully fed at a convenient diſtance of time before his being diſſected, the Veſſel ſwell'd with alimental juices would be the better diſcernable; whereupon having feaſted another Dog ſome hours before he opened him, he manifeſtly detected thoſe milky Veſſels, whoſe diſcovery has ſince ſet Anatomiſts ſo uſefully on work.

But, *Pyrophilus*, not to exceed the limits of an Eſſay, I muſt not multiply Inſtances of the Contingencies of Experiments, but content my ſelf to tell you in general, that in divers Caſes ſuch circumſtances as are very difficult to be obſerved, or ſeem to be of no concernment to an Experiment, may yet have a great influence on the Event of it. If on either of the Extremes or Poles of a good armed Load-ſtone, you leiſurely enough, or divers times, draw the back of a Knife, which has not before receiv'd any Magnetick influence, you may obſerve, that if the point of the blade have in this affriction been drawn from the middle or Æquator of the Load-ſtone towards the Pole of it, it will attract one of the Extremes of an equilibrated Magnetick Needle; but if you take another Knife

that

that has not yet been invigorated, and upon the ſelf-ſame Extremity or Pole of the Load-ſtone, thruſt the back of the Knife from the Pole towards the Æquator or middle of the Load-ſtone, you ſhall find, that the point of the Knife has, by this bare difference of Poſition in the blade whilſt it paſt upon the Extreme of the Load-ſtone, acquired ſo different a Magnetick property, or Polarity, from that which was given to the former Knife by the ſame Pole of the Load-ſtone, that it will not attract, but rather ſeem to repel or drive away that end of the Magnetick Needle which was drawn by the point of the other Knife. And this improbable Experiment not only we have made trial of, by paſſing ſlender Irons upon the Extremities of armed Load-ſtones, the breadth of whoſe Steel-caps may make the Experiment ſomewhat leſs ſtrange, but we have likewiſe try'd it by affrictions of ſuch Irons upon the Pole of a naked terella, and we found it to ſucceed there likewiſe. How ſtrange ſoever it may ſeem, that the ſame point or part of the Load-ſtone ſhould imbue Iron with contrary Properties, barely as they are, during their paſſing over it, drawn from the Æquator of the Load-ſtone, or thruſt towards it. But whether, and how far this Obſervation inſinuates the operations of the Loadſtone to be chiefly performed by ſtreams of ſmall particles, which perpetually iſſuing out of one of its Poles, do wheel about and re-enter at the other; We ſhall not now examine (though this ſeem one of the moſt likely Phænomena we have met with, to hint a probable Magnetical Hypotheſis) contenting our ſelves to have manifeſted by what plainly appears, how much influence a circumſtance, which none but a Magnetick Philoſopher would take notice of, may have on an Experiment. We have alſo with pleaſure obſerv'd, how Artificers in the tempering of Steel, by holding it but a minute or two longer or leſſer in the flame, (or other competent Heat) do give it very

differing tempers, as to brittleneſs or toughneſs, hardneſs or ſoftneſs; for as when it is taken out of the flame to be extinguiſhed, it looks either red, yellow, or blew, ſo they eſteem and find it fit to make Knives, Engraving Tools, or Springs for Watches, *&c.* and yet it paſſes from one colour to another ſo ſwiftly, that none but an Artiſt expert in tempering of Iron would ſuſpect, that ſo ſmall a difference oft-time of its ſtay in the flame could produce ſo great a difference in its tempers. On which occaſion, *Pyrophilus*, I call to mind, that making a while ſince ſome tryals concerning Gravers in the Shop of a famous Artificer, he preſented me, as a great rarity, a Graver (which I yet keep) that would make the uſual Experiments about tempering of Gravers appear falſe to him that ſhould never try them but upon it; for with all the care wherewith I try'd upon it the known ways of ſoftning Gravers, I could not ſoften this: which men eminently skilled in theſe matters (together with the perſon that made it) affirmed to have been made of *Damaſco*-Steel, the ſtrength whereof in cutting Iron I have (not without ſome wonder) made trial of. But whether this ſingularity which we have mention'd in this Graver proceed from the nature of the Steel, or from the temper that it had afterward given it, is not yet agreed upon by thoſe skilful men to whom I have ſhew'd it: but one of them, who by making Inſtruments for Navigators, has had the opportunity of making more than ordinary enquiry into matters of this nature, aſſures me, that he can eaſily ſoften this kind of Steel, by only taking it off the Fire at a certain nick of time, differing from that which is wont to be obſerved in order to the ſoftning of common Gravers. And who knows but that in many other Experiments, ſeemingly deſpicable and unheeded Circumſtances may be of great concernment, though by reaſon of the want of ſuch particular Obſervations as the frequent dealing with the ſame body has given Magnetick Phi-

Philoſophers and Artificers occaſion to make, men have not yet taken notice of their importance.

To give you one Inſtance to this purpoſe, *Pyrophilus*, let me take notice to you, that divers Planters of Fruit-trees have with wonder obſerv'd, that ſome Grafts of Cherry-trees, for example, have born fruit the ſame year that they were grafted, (nay I have obſerv'd ſome Plants to bear fruit the ſame quarter of the year) and others not till the year after their inſition, though neither in the goodneſs of the Graft, nor in that of the Stock, they had obſerved any diſparity to which the difference above mentioned could be aſcrib'd; and therefore the bearing or not bearing of the Ciens of a Cherry-tree the firſt year of its Inſition is by many Gardiners look'd upon as a thing meerly Contingent. And yet indeed it ſcarce deſerves to be reckon'd among ſuch contingent Experiments as we have been hitherto treating of; for I am inform'd by the trials of more than one of the moſt skilful and experienc'd Grafters of theſe parts, that a man ſhall ſeldom fail of having Cherries born by his Graft the ſame year in which the Inſition is made, if he take care that his Graft, which muſt be of a good kind, have bloſſom-buds, as they are wont to be call'd, upon it: Whereas if it were only leaf-buds, as they may be term'd, it will not bear fruit till the ſecond ſeaſon; and this not being taken notice of by vulgar Gardiners, makes them, as we have ſaid, impute a needleſs Contingency to the fruitfulneſs of ſuch kind of Grafts. Now to diſcern ſuch buds as are fit to produce bloſſoms from ſuch as will diſplay themſelves but in leaves, is no difficult matter, the former ſort being more full, and big, and round than the latter, which are wont alſo to lye more flat and cloſe to the Graft. And 'twas, *Pyrophilus*, ſuch obſervations as this that induc'd us after the beginning of the former Eſſays, to diſcriminate from ſuch contingent Experiments as thoſe wherein the cauſe of the Contingency

is very abſtruſe and difficult to be diſcern'd, ſuch other Experiments whoſe ſeeming Contingency proceeds from more eaſily diſcoverable cauſes; for ſuch by diligent obſervation of circumſtances may be reduc'd to a greater certainty than the others ſeem capable of. Though I dare not deny that even divers of thoſe contingent Experiments, which to us yet ſeem to belong to the firſt ſort, by mens future skill and diligence in obſervation may be made fit to be reduc'd to the ſecond ſort.

Before I leave this Subject, *Pyrophilus*, I dare not omit to ſay ſomething to you of the *Virgula Divina*, or rather *Divinatoria*, by which many Mineraliſts pretend to diſcover the latent veins of Metals. Some uſe a forked hazel, whoſe horns they hold by the ends one in each hand; and others content themſelves to chuſe a hazel rod (which ſome will have to be all of the ſame years ſhoot) and this they bind on to another ſtreight ſtick of any other wood, and walking ſoftly with it over thoſe places where they ſuſpect the bowels of the earth to be enriched with Metals, they ſay, that if they paſs over a Metalline vein, the Wand will by bowing towards it diſcover it. And ſome Dealers in Metals I know who affirm, that by holding the Metals ſucceſſively in that hand wherein a man holds the rod, he may diſcover what determinate Metal is predominant in the Vein: for when he puts into his hand that Metal wherewith the Mine chiefly abounds, the Wand will manifeſtly bow more ſtrongly than when 'tis held in the hand with any other Metal. What to determine concerning the truth of this perplexing Experiment, I confeſs I know not. For *Agricola* himſelf, after a long debate concerning it, gives us this account of his ſenſe, *Metallicus igitur* (ſays he) *quia eum virum bonum & gravem eſſe volumus, virgulâ incantatâ non utetur, quia rerum naturæ peritum & prudentem, furcatam ſibi uſui non eſſe, ſed, ut ſupra dixi, habet naturalia venarum*

De re Metallicâ lib. 2. p. 28.

rum signa quæ observat. The diligent *Kirche-rus* informs us in his *Arte magnetica*, that having exactly try'd the Experiment with Metals, for he mentions not his having try'd it with Mines, he could not find it in any measure succeed; and we our selves having several times made trial of it in the presence of the confidentest assertors of the truth of it, could not satisfie our selves that the Wand did really stand either to the Metals when placed under it, or to the Metalline Veins, when we carried it over Mines whence Metalline Oar was at that very time digging out. But on the other side diverse good Authors, and even our diligent Country-man *Gabriel Plat*, though wont to be somewhat too severe to Chymists, does ascribe very much to this detecting Wand, and diverse persons, in other things very far from credulous, have as Eye-witnesses with great asseverations asserted the truth of the Experiment before us; and one Gentleman who lives near the Lead-Mines in *Somersetshire*, leading me over those parts of the Mines where we knew that Metalline Veins did run, made me take notice of the stooping of the Wand when he passed over a Vein of Oar, and protested, that the motion of his hand did not at all contribute to the inclination of the Rod, but that sometimes when he held it very fast, it would bend so strongly as to break in his hand. And to convince me that he believed himself, he did upon the promises made him by his stooping Wand put himself to the great charge of digging in untry'd places for Mines, (but with what success he has not yet inform'd me.) Among the Miners themselves I found some made use of this Wand, and others laughed at it. And this I must take notice of as peculiar to this Experiment, that the most knowing Patrons of it confess, that in some mens hands it will not at all succeed, some hidden property in him that uses the Wand being able, as they say, to overpower and hinder its inclinatory virtue. To which I

Lib. 3. part. 3. cap. 3.

must

muſt add what a very famous Chymiſt, who affirms himſelf to have try'd many other things with it beſides thoſe that are commonly known, very ſolemnly profeſſed to me upon his own knowledge, namely, that in the hands of thoſe very perſons, in whoſe hands the Rod will (as they ſpeak) work, there are certain unlucky Hours, govern'd by ſuch Planets and Conſtellations, (which I confeſs I believ'd not enough to remember their names) during which it will not work, even in thoſe hands wherein at other times it manifeſtly will. But of this Experiment I muſt content my ſelf to ſay, what I am wont to do when my opinion is ask'd of thoſe things which I dare not peremptorily reject, and yet am not convinc'd of; namely, that they that have ſeen them can much more reaſonably believe them than they that have not.

Nor is it only in Experiments, *Pyrophilus*, but in Obſervations alſo, that much of Contingency may be, witneſs the great variety in the number, magnitude, poſition, figure, &c. of the parts taken notice of by Anatomical Writers in their diſſections of that one Subject the humane body, about which many errors would have been delivered by Anatomiſts, if the frequency of diſſections had not enabled them to diſcern betwixt thoſe things that are generally and uniformly found in diſſected bodies, and thoſe which are but rarely, and (if I may ſo ſpeak) through ſome wantonneſs or other deviation of Nature, to be met with. I remember that a while ſince being preſent at the diſſection of a luſty young Thief, we had opportunity to obſerve among other things, that the interval betwixt two of his ribs was near the back-bone fill'd up with a thick bony ſubſtance, which ſeem'd to be but an expanſion of the ribs, and appeared not to have grown there upon occaſion of any fracture, or other miſchance. About the ſame time being at a private diſſection of a large and young humane Body with ſome learned men, an ingenious Perſon Profeſſor

Profeſſor of Anatomy there preſent, chancing to cut a great Nerve, ſpy'd in the ſubſtance of it a little of a very red Liquor, which he immediately ſhew'd me, as wondring what it might be: but I concluding it to be Blood, preſently ſuſpected that it might have proceeded from ſome ſmall unheeded drop of blood wip'd off by the bruſhy ſubſtance of the Nerve from the Knife wherewith it was cut. Wherefore carefully wiping a Diſſecting Knife, I did in another place cut the Nerve aſunder, and found another very little drop of pure blood in the ſubſtance of it as before. This I did again elſewhere with like ſucceſs, ſhewing it to the by-ſtanders, who admir'd to ſee a Veſſel carrying blood (for ſuch they concluded it to be) in the body of a Nerve, in regard they remember'd not to have ever met with ſuch an accident; though I the leſs admire it, becauſe I have in an Oxes Eye or two obſerv'd in that coat which the Moderns commonly call the Retina, and which ſeems to be but an expanſion of the Pith of the Optick Nerve, little turgent veins manifeſtly full of blood.

We further obſerv'd in that lately-mention'd body, in which we took notice of the irregular conjunction of two Ribs, that the Lungs which were very ſound had a ſupernumerary lobe on one ſide, which did ſo little differ from its companions, that we did not, till we had diſplay'd the Lungs, take notice of it. And I remember that a while before, being invited by a company of Phyſitians to a private Diſſection, and the Lungs, which otherwiſe ſeemed not unſound, appearing in divers places faſtned to the ribs, two ingenious Anatomiſts that were there preſent, did ſo little agree in their Obſervations concerning ſuch caſes, that the one affirmed, that he had never ſeen any Lungs (which had not been exceſſively morbid) tied to the Thorax; and the other proteſted, that he had ſcarce ever opened a diſeaſed body wherein the Lungs did not ſo adhere. But if it were not improper to mind a young Gentleman of

Venereal

Venereal Obſervations, I could eaſily give you an eminent proof of the diſagreement of Anatomical Obſervators, by inſiſting on the Controverſie betwixt the famous Writers on that Subject, concerning the Anatomical notes or tokens of Virginity, many eminent Authors affirming, that they have ſeldom fail'd of finding them in one amongſt very many diſſected Maids; and many other Artiſts, both conſpicuous and experienced, peremptorily profeſſing, that they have ſeldom or never met with the pretended marks in perſons even of the moſt undoubted Virginity: and certainly it is very ſtrange, that about a matter which ſeems ſo eaſily determinable by Senſe, accurate and ſober men ſhould ſo widely diſagree, as that the one ſhould profeſs he has very rarely, if ever, met with in a humane body, what another affirmeth himſelf to have as ſeldom, if ever, miſs'd. But becauſe, *Pyrophilus*, this ſubject is perhaps ſomewhat improper to be inſiſted on either to, or by, a yong man, I ſhall paſs on to tell you, that amongſt the accurateſt of our modern Writers, I ſuppoſe you will readily allow me to reckon D. *Harvey* and D. *Highmore*, and that that though in their excellent Treatiſes of Generation they both inſiſt on the production and changes obſervable in Hens Eggs, as the Patterns whereunto the Generation of other Animals may be referred; yet have we diverſe times in the progreſs of Nature in her formation of a Chick, obſerved conſiderable variations in point of time and other circumſtances (though in the main our Obſervations commonly agreed) from what is by them delivered: which diverſity may eaſily proceed from the differing conſtitutions of Hens, their differing aſſiduity in ſitting on their Eggs, the differing qualifications of the Eggs themſelves, and ſeveral other particulars of like nature. And I remember, that the other day taking notice of this to my learned friend Dr. *Highmore*, he readily acknowledged to me, that he himſelf had likewiſe obſerv'd diverſe circum

ſtance-

ſtances in Eggs whilſt they were hatching, which varied from thoſe ſet down by him in his Book, though he had there accurately expreſs'd the changes he diſcerned in thoſe Eggs which at that time afforded him his Obſervations. And indeed there are certain things of ſuch a nature, that ſcarce any ſingle mans accurateneſs in making a ſingle Obſervation about them, can ſecure him from appearing unskilful or unfaithful in his Obſervations, unleſs thoſe that ſhall afterwards examine them chance to be endowed with a ſomewhat more than ordinary either equity, or ſagacity, or both. For inſtance, he that firſt affirmed that a Needle animated by a Loadſtone did conſtantly convert its extreams to the oppoſite Poles of the Earth, could ſcarce ſuſpect himſelf of having delivered any thing which he had not carefully try'd. And yet of thoſe excellent Pilots, *Gonzales Oviedo* and *Sebaſtian Cabot*, (who are ſaid to have in *America* firſt taken notice of the Declination of the Mariners Needle) he that did firſt in thoſe far diſtant parts of the world compare the Meridian Line afforded by Magnetical Needles with one Mathematically drawn (which may be readily found by accurate Sun-dials) and thereby obſerve the variation of the Needle, or its declination from the true Meridian Line, might eaſily conclude the Obſerver formerly mention'd to have been faulty, by reaſon of his finding the Needles variation differing (perhaps by divers degrees) from that delivered by the *firſt* Obſerver. And this *ſecond* mans Obſervation might appear to have been as careleſly made to a hundred other Obſervers, if the Obſervations of Navigators had not made it apparent, that the Declination of the Needle is far from being the ſame in all places: for though *Cardan* (as *Kircher* and *Fracaſtorius*, as another informs us) be pleaſed to affirm, that the Loadſtone declines as many degrees as the Pole-ſtar is diſtant from the Pole of the world; yet beſides divers

Fournier Hydr. l.11. c.11.

reaſons, common Experience ſufficiently manifeſts the inconſiderableneſs (not to ſpeak more harſhly) of that aſſertion. For about the Iſlands of the *Azores*, eſpecially that of *Corvo*, over which the firſt Meridian is by many ſuppoſed to paſs, the Magnetick Needle hath bin obſerved directly to reſpect the Poles, without any ſenſible declination from them; but in other places it is wont to vary ſometimes Eaſtward, ſometimes Weſtward, more or leſs. Inſomuch that not only our venturous Countryman Captain *Thomas James* obſerved it in 63 degrees North-Latitude to be no leſs than 27 Degrees, 48 Minutes; but a learned Mathematical Writer that is lately come forth, makes the Declination at the *Fretum Davis* to amount to what is almoſt incredible, 50 Degrees. And this Deflexion of the Needle ſometimes to one ſide of the Meridian, ſometimes to the other, happens with ſo much ſeeming irregularity, as has made both the diligent *Kircher* himſelf, and divers other Magnetick Writers, almoſt deſpair of reducing theſe kind of Obſervations to any general Hypotheſis.

In the Table annex'd to his Voyage.

To which we may add, that perhaps very few even of the exacteſt Obſervations of this nature made an Age ſince, would now appear accurate to them that ſhould try them in the ſelf-ſame places wherein, and the ſelf-ſame manner after which they were formerly made. So that the diligenteſt of thoſe Obſervers would appear to us to have been negligent, if the ſagacity of ſome of their ſucceeders had not prompted them to ſuſpect, that even in the ſame place the Needles variation may vary. And I remember, that having not long ſince enquired of an Engliſh Contriver of Mathematical Inſtruments for the uſe of Sea-men, what he had obſerved concerning this alteration of the Needles variation, he told me, that by comparing of ancient and modern Obſervations made by himſelf and other accurate Mathematicians at *London*, he had found the Declination conſtantly

conſtantly to decreaſe, and, as he conjectured, about 12 or 13 Minutes (though that methinks be much) in a year. And it will be yet more difficult to ſet down any Obſervation of this nature which will appear exact to poſterity, if that ſtrange thing be true (as it may well be) which was related to *Kircher* by a friend of his, who affirms himſelf to have obſerv'd a notable change of the Needles Variation at *Naples*, after a great *Incendium* of the neighbouring Mountain *Veſuvius*; which alteration he not abſurdly ſuſpects to have proceeded from the very great change made in the neighbouring ſubterraneal parts by that great conflagration. And it ſeems the ſame Obſervation has been taken notice of by Mathematicians elſewhere. For the learned Jeſuit *Fournier* in his French Hydrography tells us in more general terms, that ſince the *Incendiums* of *Veſuvius* the Declination (of the Needle) has notably chang'd in the Kingdom of *Naples*. The ſame Author ſomewhere delivers what (if it be true) is remarkable to our preſent purpoſe, in theſe words. There are perſons who have obſerv'd, that the ſame Needle that declin'd 5 degrees upon the ſurface of the Earth, being carried down very low into certain Caves, declin'd quite otherwiſe. I added thoſe words, *if it be true*, not to queſtion the veracity of the Author, but becauſe 'tis very poſſible the makers of the obſervation (though learned men) may have been miſtaken in it without ſuſpecting themſelves in danger of being ſo. For I ſhould ſcarce have imagin'd, unleſs my own particular obſervation had inform'd me, in how great a variety of Stones and other Foſſiles the Oar of Iron may lurk diſguis'd: ſo that 'tis no way incredible, that knowing Chymiſts themſelves, and much more Mathematicians and others, not being aware of the Obſervation of what I have newly delivered, may preſume, becauſe they ſaw not in the deep Caves above mentioned any Minerals like the vulgar Iron Oar,

Livie 11. *chap.* 10.

that there is nothing of that Metal there, when indeed there may be enough to occaſion that Deflexion of the Needle; which (eſpecially if it be ſtrongly excited) may be often drawn aſide by Iron or other Magnetick Bodies, at a greater diſtance than thoſe that have not try'd will be apt to ſuſpect: which may perhaps be the reaſon why in the little Iſland of *Ilva* (upon the coaſt of *Italy*) where they dig up Iron and ſtore of Loadſtones, of which I have ſeen in *Toſcany* of a prodigious bigneſs, there is in different, but neighbouring places, ſuch a ſtrange diſparity of the Needles variation as curious men have recorded.

Nor are Magnetical and Anatomical Obſervations the only ones which are ſubject to diſagree now and then, without the negligence of thoſe that make them: but I want time, and I fear you would want patience, to conſider at preſent as many of them as might be eaſily enumerated to you.

I ſuppoſe, *Pyrophilus*, you may have obſerved, how I in the paſt Diſcourſe have forborn to inſiſt on Medicinal Experiments, which I have purpoſely done, becauſe they are ſo many, and almoſt all of them ſubject to ſuch uncertainties, that to inſiſt on them would require much more time than my occaſions will allow me to ſpend upon this Eſſay. And indeed in Phyſick it is much more difficult than moſt men can imagine, to make an accurate Experiment: for oftentimes the ſame diſeaſe proceeding in ſeveral perſons from quite differing cauſes, will be increaſed in one by the ſame remedy by which it has been cur'd in another. And not only the conſtitutions of Patients may as much alter the effects of remedies, as the cauſes of diſeaſes, but even in the ſame Patient and the ſame diſeaſe, the ſingle circumſtance of Time may have almoſt as great an operation upon the ſucceſs of a Medicine as either of the two former particulars, as we may elſewhere have occaſion by ſundry Inſtances to manifeſt. But beſides the general uncertainty

to

to which moſt remedies are ſubject, there are ſome few that ſeem obnoxious to Contingencies of a peculiar nature: ſuch is the Sympathetick Powder, of which not only divers Phyſitians and other ſober perſons have aſſur'd me they had ſucceſsfully made tryal, but we our ſelves have thought that we were Eye-witneſſes of the operation of it; and yet not only many that have try'd it have not found it anſwer Expectation, but we our ſelves trying ſome of our own preparing on our ſelves, have found it ineffectual, and unable to ſtop ſo much as a bleeding at the Noſe, though upon Application of it a little before we had ſeen ſuch a bleeding, though violent, ſuddenly ſtopt in a perſon, who was ſo far from contributing by his Imagination to the effect of the Powder, that he derided thoſe that he ſaw apply it to ſome of the drops of his blood. Wherefore that the Sympathetick Powder & the Weapon-ſalve are never of any efficacy at all, I dare not affirm; but that they conſtantly perform what is promiſed of them I muſt leave others to believe. But making mention of remedies of this nature, though I am willing, *Pyrophilus*, to put a Period both to your trouble and my own, yet I muſt not omit to tell you, that whereas the *Pæony*-root has been much commended both by ancient and modern Phyſitians of no mean account, as an Amulet againſt the Falling-ſickneſs, and yet has been by many found ineffectual, we have been apt to ſuſpect, that its inefficacy, if it be but infrequent, might poſſibly proceed from its having been unſeaſonably gather'd; and when I was laſt in the Weſt of *Ireland*, acquainting the eminenteſt of the Galeniſts there with my Conjecture, he confirm'd me in it, by aſſuring me that he had often try'd the *Pæony*-root unſeaſonably gather'd without ſucceſs, but having lately gather'd it under its proper Conſtellation, as they ſpeak, (which is when the decreaſing Moon paſſes under *Aries*) and ty'd the ſlit Root about the Necks and Arms of his

Patients,

Patients, he had freed more than one, whom he nam'd to me, from Epileptical fits. Agreeable whereunto I find, that a famous Phyſitian of *Grenoble*, *Monſieur des Grands Prez*, in the laſt of his Obſervations communicated to the famous Practical Phyſitian *Riverius*, ſolemnly profeſſes his having divers times freed his Patients from the Falling-ſickneſs by the ſingle outward application of *Pæony*-roots, collected and apply'd as is above-mention'd. But though he thence infers the uſefulneſs of obſerving the Stars in the practice of Phyſick, yet before much weight be laid upon ſuch improbable Notions as moſt of thoſe of judiciary Aſtrologers, the Influence of Conſtellations upon Simples, &c. ought by ſevere and competent Experiments to be better made out than hitherto it has been.

But to ſay no more of the contingent Obſervations to be taken notice of in tryals Medical, I could tell you that I have obſerved even Mathematical Writers themſelves to deliver ſuch Obſervations as do not regularly hold true. For though it hath been look'd upon as their priviledge and glory to affirm nothing but what they can prove by no leſs than Demonſtration; and though they uſe to be more attentive and exact than moſt other men in making almoſt any kind of Philoſophical Obſervation; yet the certainty and accurateneſs which is attributed to what they deliver, muſt be reſtrain'd to what they teach concerning thoſe purely-Mathematical Diſciplines, Arithmetick and Geometry, where the affections of Quantity are abſtractedly conſider'd: but we muſt not expect from Mathematicians the ſame accurateneſs when they deliver Obſervations concerning ſuch things wherein 'tis not only Quantity and Figure, but Matter, and its other Affections, that muſt be conſider'd. And yet, leſs muſt this be expected when they deliver ſuch obſervations as, being made by the help of material Inſtruments fram'd by the hands and tools of men, cannot but in divers caſes be ſubject to ſome

if

if not many Imperfections upon their account. Divers of the modern Aſtronomers have ſo written of the ſpots and more ſhining parts or (as they call them) *Faculæ*, that appear upon or about the ſurface of the Sun, as to make their Readers preſume, that at leaſt ſome of them are almoſt always to be ſeen there. And I am willing to think, that it was their having ſo often met with ſuch Phænomena in the Sun that made them write as they did. And yet when I firſt apply'd my ſelf to the contemplation of theſe late diſcoveries, though I wanted neither good Teleſcopes, nor a dark room to bring the ſpecies of the Sun into, yet it was not till after a great while, and a multitude of fruitleſs Obſervations made at ſeveral times, that I could detect any of theſe Solary ſpots, which have during many months at leaſt appear'd ſo much ſeldomer than it ſeems they did before, that I remember a moſt ingenious Profeſſor of Aſtronomy, excellently well furniſh'd with Dioptrical Glaſſes, did about that time complain to me, that for I know not how long he had not been able to ſee the Sun ſpotted. And as for the *Faculæ* that are written of as ſuch ordinary Phænomena, I muſt profeſs to you, *Pyrophilus*, that a multitude of Obſervations made with good Teleſcopes at ſeveral places and times whilſt the Sun was ſpotted, has ſcarce made me ſee above once any of the look'd for Brightneſſes.

And as the nature of the Material Object wherewith the Mathematician is converſant, may thus deceive the Expectations grounded on what he delivers; ſo may the like happen by reaſon of the Imperfection of the Inſtruments which he muſt make uſe of in the ſenſible obſervations whereon the mixt Mathematicks (as Aſtronomy, Geography, Opticks, &c.) are in great part built. This is but too manifeſt in the diſagreeing Supputations that famous Writers, as well Modern as Ancient, have given us of the circuit of the Terreſtrial Globe, of the diſtance and

bigneſ

bigneſs of the fix'd Stars and ſome of the Planets, nay, and of the height of Mountains: which Diſagreement, as it may oftentimes proceed from the differing Method and unequal skill of the ſeveral Obſervers, ſo it may in divers caſes be imputed to the greater or leſs exactneſs and manageableneſs of the Inſtruments employ'd by them. And on this occaſion I cannot omit that ſober Confeſſion and Advertiſement that I met with in the noble *Tycho*, who having laid out beſides his time and induſtry much greater ſums of money on Inſtruments than any man we have heard of in latter times, deſerves to be liſten'd to on this Theme, concerning which he has (among other things) the following paſſage: *Facile* (ſays he) *lapſus aliquis penè inſenſibilis in Inſtrumentis etiam majoribus conficiendis ſubrepit, qui inter obſervandum aliquot ſcrupulorum primorum jacturam faciat; inſuper ſi ipſe ſitus & tractandi modus non tamabſoluta norma perficiatur ut nihil prorſus deſideretur, intolerabilis nec facile animadvertenda deviatio ſeſe inſinuat. Adde quod inſtrumenta uſu & ætate à prima perfectione degenerent. Nihil enim quod hominum manibus paratur ab omni mutatione undiquaque exiſtit. Organa enim ejuſcemodi niſi è ſolido metallo affabre elaborentur, mutationi aereæ obnoxia ſunt; & ſi id quoque detur ut è metallica materia conſtent, niſi ingentia fuerint, diviſiones minutiſſimas graduum non ſufficienter exhibent, dumque hoc præſtant, ſua magnitudine & pondere ſeipſa ita aggravant, ut facile tum extra planum debitum aut figuram competentem dum circumducuntur declinent, tum etiam ſua mole intractabilia redduntur. Quare magis requiritur in Inſtrumentis Aſtronomicis quæ omni vitio careant conſtruendis, artificium pari judicio conjunctum, quam hactenus à quamplurimis animadverſum eſt. Id quod nos ipſe uſus longaque docuit Experientia non parvo labore nec mediocribus ſumptibus comparata.*

Tycho Brahe lib. 2. de Cometa An. 1577. p. 133.

Hitherto our noble Author. And as for the obſerva-

tions

tions made at Sea, the diligent *Fournier* advertiſes, that however many Sea-captains and others may brag of their Mathematical Obſervations made on Ship-board, yet he, upon tryal of many Inſtruments both at Sea and aſhore, makes bold to affirm, that no Aſtronomer in the world can be ſure to make his Obſervation at Sea within ten Minutes of the preciſe truth, no not (ſays he) upon the Sand it ſelf within one Minute of it.

But inſtead of acquainting you with what may be drawn from the writings of our Hydrographer, to prove that his Aſſertion is rather modeſt than too bold, I ſhall obſerve, that the Obſervations even of skilful Mathematicians may hold ſo little, or diſagree ſo much, when they pretend to give us the determinate *meaſures* of things, that I remember of three very eminent modern Mathematicians, who have taken upon them by their Experiments to determine the proportion betwixt Air and Water, the one makes not the weight of Water to exceed above a 150 times that of Air, the other reckons Water to be between 13 and 14 hundred times, and the third no leſs than 10000 times the heavier. Not to mention a modern and famous Writer or two, who have been ſo miſtaken as to think, that the weight of the Water in compariſon of the Air is I know not how much under-reckon'd even by this laſt (over-bold) Eſtimate. And if I had leiſure I could annex an Experiment partly ſtatical, and relating to the weight of the Air, which though we made divers times in an hour, yet we miſs'd of the like ſucceſs twice as often in the ſame hour, without being able to know before-hand whether the Experiment would ſucceed within ſome pounds weight. But of this more perhaps elſewhere.

The Ends, *Pyrophilus*, which we have propoſed to our ſelves in ſetting down the things by us deliver'd in this and the former Eſſay, are principally two.

And firſt, we deſire that the Inſtances we have given you

of the Contingency of Experiments may make you think your ſelf oblig'd to try thoſe Experiments very carefully, and more than once, upon which you mean to build conſiderable Superſtructures either theorical or practical, and to think it unſafe to rely too much upon ſingle Experiments, eſpecially when you have to deal in *Minerals*: for many to their ruine have found, that what they at firſt look'd upon as a happy Mineral Experiment has prov'd in the iſſue the moſt unfortunate they ever made. And I remember that the moſt experienc'd Mineraliſt I have hitherto been acquainted with, though his skill has been rather gainful then prejudicial to him, has very ſeriouſly told me, that he could quickly grow an extraordinary rich man, if he could but do conſtantly whatſoever he has done, not only two or three, but many times.

The other End, *Pyrophilus*, to which I had an Eye in writing the paſt Diſcourſes, was, that they may ſerve for a kind of Apology for Sober and Experimental Writers, in caſe you ſhould not always upon tryal find the Experiments or Obſervations by them deliver'd anſwer your expectations. And indeed it would prove a great diſcouragement to wary and conſiderate Naturaliſts from enriching the World with their Obſervations, if they ſhould find, that their faithfulneſs in ſetting down what they obſerved is not able to protect them from blaſting imputations of falſhood, but that by publiſhing any thing for the good of others, they muſt expoſe their reputation to all the uncertainties to which any of their Experiments may chance to prove obnoxious. 'Tis true indeed, that if a Writer be wont to be fabulous or tranſcriptive, and to deliver things confidently by hear-ſay without telling his Readers when he does ſo, if his Experiments upon tryal ſucceed not, we may be allowed to impute their unſucceſsfulneſs rather to him than to our ſelves or to chance, and need not think our ſelves obliged to have ſo much a greater

care

care of his reputation than he had of his own, as for his ſake to try more than once what he for our ſakes never try'd ſo much as once. But if an Author that is wont to deliver things upon his own knowledge, and ſhews himſelf careful not to be deceived, and unwilling to deceive his Readers, ſhall deliver any thing as having try'd or ſeen it, which yet agrees not with our tryals of it; I think it but a piece of Equity, becoming both a Chriſtian and a Philoſopher, to think (unleſs we have ſome manifeſt reaſon to the contrary) that he ſet down his Experiment or Obſervation as he made it, though for ſome latent reaſon it does not conſtantly hold; and that therefore though his Experiment be not to be rely'd upon, yet his ſincerity is not to be rejected. Nay, if the Author be ſuch an one as has intentionally and really deſerved well of Mankind, for my part I can be ſo grateful to him, as not only to forbear to diſtruſt his Veracity, as if he had not done or ſeen what he ſays he did or ſaw, but to forbear to reject his Experiments, till I have tryed whether or no by ſome change of Circumſtances they may not be brought to ſucceed. Thus a while ſince finding in Sir *Francis Bacon*, that he delivers as a ſomewhat unlikely truth, that Spirit of Wine will ſwim upon Oyl (of Almonds) we forthwith made tryal of it, but found the Oyl ſwim upon the Spirit of Wine, and this upon ſeveral tryals before Witneſſes: but our tenderneſs of the reputation of ſo great and ſo candid a Philoſopher made us to bethink our ſelves, that (though he mentions it not, nor perhaps thought of any ſuch thing, yet) poſſibly he may have uſed Spirit of Wine more pure than ordinary; and thereupon having provided ſome that was well rectifi'd, we found that the Oyl that was wont to ſwim upon Spirit of Wine, not freed from its aqueous parts, did readily ſink, and quietly lye in the bottom of that which was carefully dephlegm'd. And ſo having been inform'd that the learned Dr. *Brown* ſomewhere de-

livers, that *Aqua fortis* will quickly coagulate common Oyl, we pour'd ſome of thoſe Liquors together, and let them ſtand for a conſiderable ſpace of time in an open Veſſel, without finding in the Oyl the change by him promiſed, (though we have more than once with another Liquor preſently thickned common Oyl.) Whereupon being unwilling that ſo faithful and candid a Naturaliſt ſhould appear fit to be diſtruſted, we did again make the tryal with freſh Oyl and *Aqua fortis* in a long-neck'd Vial left open at the top, which we kept both in a cool place, and after in a digeſting Fornace; but after ſome weeks we found no other alteration in the Oyl than that it had acquir'd a high and lovely tincture: notwithſtanding which being ſtill concern'd for the reputation of a perſon that ſo well deſerves a good one, the like Contingencies we have formerly met with in other Experiments, made us willing to try whether or no the unſucceſsfulneſs we have related might not proceed from ſome peculiar though latent Quality, either in the *Aqua fortis* or the Oyl by us formerly employ'd; whereupon changing thoſe Liquors, and repeating the Experiment, we found after ſome hours the Oyl coagulated almoſt into the form of a whitiſh Butter. Nor dare I allow my ſelf to be confident, that I ſhall not need to be dealt with by you upon ſome occaſions with the like equity that I have been careful to expreſs towards others. And ſince the writing of thus much of this very Eſſay, having deſir'd a very skilful and candid Chymiſt to do me the favour to provide me ſome of the pureſt and ſtrongeſt Spirit of Salt that could be made; he kept ſome Salt in a vehement fire for divers days and nights together, and freed the extracted Liquor ſo carefully and ſo skilfully both from its phlegm and its terreſtrial *fæces*, that after all I have written in the former Eſſay concerning that Menſtruum, I muſt freely confeſs to you, that I am now ſatisfi'd, that a Spirit of Sea-ſalt may without any unſincerity be

be ſo prepar'd as to diſſolve the body of crude Gold, though I could not find that the Solutions I made of that Metal were red, but rather of a yellow or golden colour, much like thoſe made with common *Aqua Regis*. But neither this Artiſt nor I have been ſince able to make another Spirit of Salt capable of diſſolving Gold, notwithſtanding all the induſtry we have employ'd about it, which makes me refer this to Contingent Experiments; unleſs the proſperous event of our former tryal may be aſcrib'd to the quality of the Salt that was diſtill'd, which was brought from the Iſland of *Mayo*, where the ſcorching Sun makes out of the Sea-water a Salt that is accounted much ſtronger and more ſpiritous than that which is wont to be made in *France* and other more temperate Climats. And let me, *Pyrophilus*, take this opportunity to add, that if I had not very cautiouſly ſet down the Obſervation I related in another Eſſay * concerning the little Fiſhes or Worms I there teach you to diſcover in Vinegar, I ſhould perhaps need much of your equity to keep me from being thought to have impos'd upon you in what I there delivered. For I have ſince met with divers parcels of Vinegar wherein the Obſervation could not be made, for one wherein it held; ſo that I am glad to keep by me ſome Vinegar ſtock'd with thoſe ſcarce viſible Animals to ſatisfie ingenious men, among whom ſome have been fain, after their own fruitleſs tryals, to come to me to ſhow them the things delivered in that Obſervation. What I mention'd a little above to have been try'd upon Sallet-oyl, puts me in mind of telling you, that among our Experiments concerning the changes of colours, we were about to acquaint you with one which we had formerly made upon common Oyl-Olive, it ſeeming to us a not inconſiderable one, ſince it was a way that we devis'd of inſtantly changing the colour of the Oyl

* This is one of thoſe that make up the Book of the Uſefulneſs of *Experimental Philoſophy*.

Oyl from a pale Yellow to a deep Red, with a few drops of a Liquor that was not red but almoſt colourleſs. This Experiment, as we were ſaying, *Pyrophilus*, we were about to ſet down among others concerning Colours, but becauſe we do not willingly rely on a ſingle tryal of ſuch things as we know not to have been ever try'd before, we thought it not amiſs for greater ſecurity to make the Experiment the ſecond time, but could not then find it to ſucceed, nor even ſince upon a new Trial (probably by reaſon of ſome peculiar quality in that particular parcel of Liquor we firſt made uſe of) which made us think fit to omit the intended mention of it; but if I had upon my firſt trial acquainted you with it without any further ſcruple, you might upon trial have ſuſpected, if not concluded, that I had miſinform'd you, though I had really deliver'd nothing but what I had try'd. And indeed, *Pyrophilus*, though I have not the vanity to pretend to have deſerv'd ſo much of you as ſuch Naturaliſts as Sir *Francis Bacon* have deſerv'd from every ingenious Reader of their Books; yet perhaps you will do me but Right to believe, that though ſome of the Experiments I have deliver'd may prove Contingent, yet I have not deliver'd them unfaithfully in reference to what I thought I obſerved in them and remembred of them. And though I deſire you ſhould ſo read my writings as to give no farther aſſent to my Opinions than the reaſons or Experiments produc'd on their behalf require, yet in matters of fact which I deliver as having try'd or ſeen them, I am very willing you ſhould think, that I may have had the weakneſs to be miſtaken, but not an intention to deceive you.

There is yet one thing more that I ſhall venture to acquaint you with before I conclude this Eſſay, though you may think it reliſhes of a Paradox, and it is this: That when I am ſatisfi'd of the Abilities and Circumſpection of a Writer, delivering a matter of fact as upon his own know-

knowledge; I do not presently reject his Observation as untrue, much less condemn the Person himself as a Lyer, whensoever I find that it seems to be contradicted by a contrary and more undoubted Observation, or to contradict a receiv'd and plausible either Hypothesis or Tradition: but rather try if by fit Distinction or Limitation I can reconcile them; unless I can imagine something or other which might probably lead him to mistake. And of this indulgence to an intelligent Writer I have this reason to give, That *sometimes* there happen irregularities contrary to the usual course of things, as is evident in Monsters; and *sometimes* the received Hypothesis, though perhaps not to be rejected as to the main, will not hold so universally as men presume; and *sometimes* too the contradiction betwixt the Observations may be but *seeming* (by reason of the want of some unheeded Circumstance necessary to make them inconsistent) and so they may both be true.

We might dilucidate and confirm what we have newly delivered by several Instances, were it not that this Essay is already but too prolix. Wherefore we shall only recommend to your Consideration these few Particulars.

That the Irish Spiders (of which, whatever is vulgarly believ'd to the contrary, my self have in *Ireland* seen divers) are not poisonous is not doubted by the Inhabitants, who have had many Ages experience of their harmlesness: and yet I dare not deny what the learned *Scaliger* somewhere affirms, that in (his Country if I mis-remember not) *Gascony* their venom is so pernicious, that they sometimes poison those that tread upon them through the very soles of their shoos. And that even here in *England* (though a Country so near to *Ireland*) *some* Spiders (at least) are venomous even without biting, I may elsewhere have occasion to give you an experimental proof.

It is so much taken for granted by divers Authors, who pretend likewise to give reasons of it, and by the generality

lity of their Readers, that under the ſame Meridian the magnetick Needle keeps every where the ſame variation without changing it by being carried Northwards or Southwards, that 'tis like if many Perſons better acquainted with Magnetick Speculations than Trials, ſhould read in the relations of the Hollanders, that under the Meridian that paſſes by the Iſland of *Corvo*, where the Needle points directly at the Poles, and which is therefore wont to be reckon'd the firſt Meridian, they found at two places, the one about 46, the other about 55 Degrees of Northern Latitude, a Declination in the former of thoſe Elevations of no leſs than 7 or 8 Degrees, and in the latter of a far greater number; and alſo that they found under the 20. Parallel of Southern Latitude under the ſame Meridian of the *Azores* 10 or 11 Degrees of Declination; many, I ſay, if they ſhould meet with theſe particulars, probably would ſuppoſe the Dutch to have been very bad Obſervers, becauſe theſe Obſervations do not (as we intimated above) agree with the Theory of the Needles Declination. And yet if we confer theſe Obſervations with others of the like nature made by good Navigators and other skilful men along other Meridians, we may, I ſuppoſe, find cauſe rather to rectifie the general opinion than reject the Dutch Obſervations for their diſagreeing with it, eſpecially if we take into conſideration what is affirmed by the Jeſuite *Jules Alenis* (whom *Fournier*, amply treating of Longitudes, extols for the accurateſt Obſerver of the Needles variation that ever was) ſailing into *China* in a great *Portugal* Carraque, and accompanied by the famous Pilot *Vincent Rodrique*, who had then made 28 Voyages to the *Indies*. For out of one of this Fathers Letters *Fournier* has preſerved this memorable paſſage,

De la Longitude chap. xxxiv.

You muſt (ſays he) *take notice of one thing very conſiderable, namely, that the further you go from the Æquator in the ſame Meridian, the greater*

greater you will find the Magnetical variation. There are ſome eminent modern Naturaliſts who affirm, that they have aſſuredly try'd by Weather-glaſſes, that Cellars and other Subterraneal places are colder in Winter than in Summer: and yet not to oppoſe to this Experiment the common Tradition to the contrary, I remember, that the bold and induſtrious Captain *James* (formerly mention'd) in the relation of his ſtrange Voyage publiſhed by his late Majeſties command, has this notable Obſervation, where he relates the exceſſive coldneſs of the water they met with in Summer in that Icy Region where they were forced to Winter in the Year 1632. *Moreover our Well* (ſays he) *out of which we had water in* December, *had none in* July.

Laſtly, though in the Weſtern parts it have been obſerved, that generally the inſide or heart as they call it of Trees, is harder than the outward parts, yet an Author very well vers'd in ſuch matters treating of the building of Ships, gives it us for a very important Advertiſement touching that matter, that they have obſerved at *Marſeilles*, and all along the Levantine ſhores, that that part of the Wood that is next the Bark is ſtronger than that which makes the heart of the Tree. But to draw at length to a concluſion of this already too tedious Eſſay; The Ends above mention'd, *Pyrophilus*, being thoſe which I propos'd to my ſelf in writing the paſt Diſcourſe, you will make an uſe of it, which I was very far from intending you ſhould, if you ſuffer it to diſcourage you from the vigorous proſecution of your Enquiries into Experimental Knowledge: nor indeed is any thing that hath been ſaid fit to perſwade you to other than Watchfulneſs in obſerving Experiments, and Warineſs in relying on them, but not at all to ſuch a deſpondency of mind as may make you forbear the proſecution of them: for neither doth

Fourn. Architecture Navale chap. 22.

the Phyſician renounce his Profeſſion, becauſe divers of the Patients he ſtrives to cure are not freed from their diſeaſes by his Medicines, but by death; nor doth the painful Husbandman forſake his Cultivating of the ground, though ſometimes an unſeaſonable ſtorm or flood ſpoils his Harveſt, and deprives him of the expected fruit of his long toils. For as in Phyſick and Husbandry, thoſe that exerciſe them are kept from deſerting their profeſſions, by finding, that though they ſometimes miſs of their Ends, yet they oftentimes attain them, and are by their Succeſſes requited not only for thoſe Endeavours that ſucceed, but for thoſe that were loſt: ſo ought we not by the Contingencies incident to Experimental attempts to be deterr'd from making them, becauſe not only there are many Experiments ſcarce ever obnoxious to caſualties, but even among thoſe whoſe event is not ſo certain, you may very probably make an Experiment very often without meeting with any of thoſe unlucky accidents which have the power to make ſuch Experiments miſcarry; and ſure the proſperous ſucceſs of many ſucceeding attempts is well able to make amends for the fruitleſs pains employ'd on thoſe few that ſucceed not; eſpecially ſince in Experiments it not unfrequently happens, that even when we find not what we ſeek, we find ſomething as well worth ſeeking as what we miſt. Of this laſt-mention'd truth we may elſe-where have occaſion to diſcourſe more largely, and therefore ſhall now conclude with barely minding you, that even Merchants themſelves are not wont to quit their profeſſion, becauſe now and then they loſe a Veſſel at Sea, and oft-times their Ships are by contrary winds and other accidents forc'd to put in at other Ports than thoſe they were bound for. Which example I the rather make uſe of, becauſe that as the *American* Navigators employ'd by the *European* Merchants having been by ſtorms forc'd from their intended courſe, have been ſometimes

times thereby driven upon unknown Coaſts, and have made diſcovery of new Regions much more advantagious to them than the faireſt and conſtanteſt winds and weather could have been; ſo in Philoſophical Trials, thoſe unexpected accidents that defeat our endeavours do ſometimes caſt us upon new diſcoveries, of much greater advantage than the wonted and expected ſucceſs of the attempted Experiment would have proved to us.

SOME

SPECIMENS

OF AN

ATTEMPT

To make

CHYMICAL EXPERIMENTS

Useful to Illustrate the Notions of the

CORPUSCULAR PHILOSOPHY.

LONDON,

Printed for *Henry Herringman*, *Anno* 1668.

SOME

SPECIMENS

OF AN

ATTEMPT

To make

CHYMICAL EXPERIMENTS

Usefull to Illustrate the Notions of the

CORPUSCULAR PHILOSOPHY.

LONDON,

Printed for Henry Herringman, June 1658.

THE PREFACE,

Giving an account of the two following Treatiſes, and Propoſing the Deſirableneſs of a good Intelligence betwixt the Corpuſcularian Philoſophers and the Chymiſts.

THere are many Learned Men, who being acquainted with Chymiſtry but by report, have from the Illiterateneſs, the Arrogance and the Impoſtures of too many of thoſe that pretend skill in it, taken occaſion to entertain ſo ill an opinion as well of the Art as of thoſe that profeſs it, that they are apt to repine when they ſee any Perſon capable of ſucceeding in the ſtudy of ſolid Philoſophy, addict himſelf to an Art they judge ſo much below a Philoſopher, and ſo unſerviceable to him: Nay, there are ſome that are troubled when they ſee a Man acquainted with other Learning countenance by his Example ſooty Empiricks, and a ſtudy which they ſcarce think fit for any but ſuch as are unfit for the rational and uſeful parts of Phyſiology. I now take notice of theſe things, becauſe they gave occaſion to the two following Treatiſes. For perceiving divers years ago, that ſome Learned Men of the temper above deſcrib'd thought it ſtrange (if not amiſs alſo) that one of whoſe ſtudies they were pleas'd to have too favourable an Expectation, ſhould ſpend upon Chymical tryals (to which I then happen'd to be invited by the opportunity of ſome Furnaces and ſome leiſure) much of thoſe Endeavours

deavours which they ſeem'd to think might be far more uſefully employ'd than upon ſuch an empty and deceitful ſtudy; perceiving this, I ſay, I thought it not amiſs to endeavour to manifeſt, that without ſeeking after the Elixir that Alchymiſts generally hope and toyl for, (but which they that knew me knew to be not at all in my aim) I did not in the Proſecution of Chymical tryals do any thing either without an end, or unſuitable to the Deſign I had of attempting to promote Mens Knowledge of the works of Nature, as well as their Power over them. In order to this, I did not think it enough to ſhew, that by an Inſight into Chymiſtry one may be enabl'd to make ſome Meliorations (I ſpeak not of Tranſmutations) of Mineral and Metalline Bodies, and many excellent Medicines for the Health of Men, beſides divers other Preparations of good uſe in particular Trades, and in ſeveral Occurrences of Humane Life; I did not, I ſay, think it enough to do this, becauſe that though this might ſuffice to evince that a rational man might without loſing his time employ ſome of it to underſtand and promote Chymiſtry; yet this would ſcarce ſuffice to manifeſt it to be uſeful to Philoſophy. And therefore there ſeem'd requiſite ſome ſpecimens, which might ſhew that Chymical Experiments might be very aſſiſtant even to the ſpeculative Naturaliſt in his Contemplations and Enquiries.

See the Eſſay of the Uſefulneſs of Chymiſtry in the II. Sect. of the I. Part Of the Uſefulneſs of E. P.

But againſt my attempting any thing of this Nature, three Difficulties oppos'd themſelves. The firſt was the want of Leiſure, in regard I was already pre-engag'd to write of other Subjects, and to proſecute ſome Experiments, whoſe event I was concern'd to know. Another Impediment was, that for other Reaſons elſewhere mention'd, and chiefly to keep my Judgment as unprepoſſeſs'd as might be with any of the Modern Theories of Philoſophy, till I were provided of Experiments to help me to judge of them, I had purpoſely refrain'd from acquainting my ſelf throughly with the intire Syſtem of either the

the Atomical, or the Carteſian, or any other whether New or Reviv'd Philoſophy; and therefore I could ſcarce be fit to ſhew how Chymical Experiments might illuſtrate their Doctrines. And thirdly, ſome of thoſe Learned Men for whom I was to write, more favouring the Epicurean, and others (though but a few) being more inclinable to the Carteſian opinions, it ſeem'd very difficult to gratifie by the ſame Compoſures Perſons of differing Perſwaſions.

But as to the firſt of theſe Diſcouragements, ſince my Pre-engagements to other Themes were not unknown to thoſe for whom I was to write, it might reaſonably be preſum'd they would over-look ſuch unaccurateneſs as ſhould appear imputable to haſte: And beſides, ſome ſuch Subject might be choſen to write of, as would conveniently admit Enlargements and Additions, according as my leiſure ſhould afterwards ſerve me to annex them

On occaſion of the ſecond Impediment, I remember'd, that having divers years before read the Lives of the Atomical, among other Philoſophers, in Diogenes Laertius, *and having ſometimes occaſionally heard mention made of divers Epicurean and Carteſian Notions, and having hence fram'd to my ſelf ſome general, though but imperfect, Idea of the way of Philoſophizing my friends eſteem'd; I thought I might without a more particular and explicit Enquiry into it, ſay ſomething to illuſtrate ſome Notions of it, by making choice of ſuch as, being of the more ſimple and obvious, did not require skill in the more myſterious points of the Hypotheſis they belong'd to.*

And as for the laſt of the three Diſcouragements above mention'd, I conſider'd, that the Atomical & Carteſian Hypotheſes, though they differ'd in ſome material points from one another, yet in oppoſition to the Peripatetick and other vulgar Doctrines they might be look'd upon as one Philoſophy: For they agree with one another, and differ from the Schools in this grand & fundamental point, that not only they take care to explicate things intelligibly; but that whereas thoſe other Philoſophers give only a general and ſuperficial account of the Phænomena of Nature

from certain ſubſtantial Forms, *which the moſt ingenious among themſelves confeſs to be Incomprehenſible, and certain* real Qualities, *which knowing men of other Perſwaſions think to be likewiſe Unintelligible; both the Carteſians and the Atomiſts explicate the ſame Phænomena by* little Bodies *variouſly figur'd and mov'd. I know that theſe two Sects of Modern Naturaliſts diſagree about the Notion of Body in general, and conſequently about the Poſſibility of a true Vacuum, as alſo about the Origine of Motion, the indefinite Diviſibleneſs of Matter, and ſome other points of leſs Importance than theſe: But in regard that ſome of them ſeem to be rather Metaphyſical than Phyſiological Notions, and that ſome others ſeem rather to be requiſite to the Explication of the firſt Origine of the Univerſe, than of the Phænomena of it in the ſtate wherein we now find it; in regard of theſe, I ſay, and ſome other Conſiderations, and eſpecially for this Reaſon, That both parties agree in deducing all the Phænomena of* Nature *from Matter and local Motion; I eſteem'd that notwithſtanding theſe things wherein the Atomiſts and the Carteſians differ'd, they might be thought to agree in the main, and their Hypotheſes might by a Perſon of a reconciling Diſpoſition be look'd on as, upon the matter, one Philoſophy. Which becauſe it explicates things by Corpuſcles, or minute Bodies, may (not very unfitly) be call'd Corpuſcular; though I ſometimes ſtyle it the Phœnician Philoſophy, becauſe ſome ancient Writers inform us, that not only before* Epicurus *and* Democritus, *but ev'n before* Leucippus *taught in* Greece, *a Phœnician Naturaliſt was wont to give an account of the Phænomena of* Nature *by the Motion and other Affections of the minute Particles of Matter. Which becauſe they are obvious and very powerful in Mechanical Engines, I ſometimes alſo term it the Mechanical Hypotheſis or Philoſophy.*

By ſuch conſiderations then, and by this occaſion, I was invited to try whether without pretending to determine the abovemention'd controverted points, I could by the help of the Corpuſcular Philoſophy, in the ſenſe newly given of that Appellation,

on, aſsociated with Chymical Experiments, explicate ſome particular Subjects more intelligibly than they are wont to be accounted for, either by the Schools or the Chymiſts. And however ſince the vulgar Philoſophy is yet ſo vulgar, that it is ſtill in great requeſt with the Generality of Scholars; and ſince the Mechanical Philoſophers have brought ſo few Experiments to verifie their Aſſertions, and the Chymiſts are thought to have brought ſo many on the behalf of theirs, that of thoſe that have quitted the unſatisfactory Philoſophy of the Schools, the greater Number dazl'd as it were by the Experiments of Spagyriſts, have imbrac'd their Doctrines inſtead of thoſe they deſerted; For theſe Reaſons, I ſay, I hop'd I might at leaſt do no unſeaſonable piece of ſervice to the Corpuſcular Philoſophers, by illuſtrating ſome of their Notions with ſenſible Experiments, and manifeſting that the things by me treated of, may be at leaſt plauſibly explicated without having recourſe to inexplicable forms, real Qualities, the four Peripatetick Elements, or ſo much as the three Chymical Principles.

Being once reſolv'd to write ſome ſuch Specimina as I formerly judg'd requiſite, I ſoon bethought my ſelf of the Experiment hereafter deliver'd concerning Salt-Petre, divers of whoſe Phænomena I did alſo, as time would permit, caſt into one of the Eſſays I was then engag'd to write to a Friend. And having diſpatch'd that little Treatiſe, it found ſo favourable a Reception among thoſe Learned Men into whoſe hands it came, that I was much encourag'd to illuſtrate ſome more of the Doctrines of the Corpuſcular Philſoſophy, by ſome of the Experiments wherewith my Furnaces had ſuppli d me; which alſo as occaſion ſerv'd I did, partly by writing ſome Phyſico-Chymical Treatiſes, and partly by making ſuch large Notes on the Eſſay concerning Salt-Petre, as might plentifully contribute to the Hiſtory of Qualities, of which I had ſometimes thoughts. And this continu'd, till in the year before the laſt, the publick Confuſions in this (then unhappy) Kingdom reducing me to quit my former Deſign, together *Vix. A. D. 1659.*

ther with the place where my Furnaces, my Books, and my other Accommodations were, I fell afterwards upon the making of Pneumatical tryals, whereof I lately ventur'd to give the Publick an account in a Book of New Experiments Phyſo-mechanical *about the Air.*

I ſhould not trouble the Reader with ſo prolix a Preface to ſuch ſmall Treatiſes as thoſe whereto this is prefix'd, but for theſe two Reaſons. The one, that I hope the fore-going Narrative will make me be the more favourably judg'd by the Philoſophers I deſire to ſerve, if ſometimes I write leſs skilfully of their Opinions than perhaps I ſhould have done had I allow'd my ſelf to ſearch into them: And the other, that I am earneſtly ſollicited to publiſh ſome other Tracts, tending to the ſame purpoſe that theſe do; to which alſo ſhould I ever be induc'd, by the Reception theſe may meet with, to trouble the World with them, the ſame Preface as it is now penn'd may ſerve for an Introduction. I had almoſt forgot to take notice, That whereas at the end of the Eſſay concerning Salt-Petre I mention'd a then newly-publiſh'd Treatiſe of the laborious Glauber's, *which I had not then perus'd, I found it to contain ſome Obſervations concerning the Hiſtory of Salt-Petre, which, if they be true, are conſiderable enough: I muſt again recõmend the examination of them to the Readers Curioſity, having been hinder'd by divers Avocations from ſaving him that labour my ſelf. And whereas alſo ſome years after I was inform'd of another little Book he had put out ſince the former, wherein he teaches us a way of purifying Salt-Petre, to make a Conjunction of the ſpirituous and fixter parts of it, and then to ſuffer the Mixture to evaporate and ſo cryſtallize into* Nitre; *This would I confeſs have made me apprehenſive of paſſing for a Plagiary with thoſe that did not know me, but that it was eaſie for me to clear my ſelf by the Teſtimony of very Learned Men, who had ſome years before perus'd my Treatiſe, and eſpecially of one perſon, (well known by his Writings) who was pleas'd to like it ſo well, as to deſire he might tranſlate it, and had*

had accordingly long before turn'd it into very elegant Latin. I might perhaps venture to adde, that though I could not juſtifie my ſelf by ſo convincing a proof of my Innocence, yet he that ſhall take the pains to conſider, that I could not borrow of Glauber the various Phænomena I have particularly ſet down, and much leſs the Reflections on them, & ſhall compare in what differing manners, and to what differing purpoſes, we two propoſe the making of Salt-Petre out of its own Spirit, and fixt Salt (He but preſcribing as a bare Chymical Purification of Nitre, what I teach as a Philoſophical Redintegration of it;) He, I ſay, who ſhall compare theſe things together, will perchance think, that I was as likely to find this laſt nam'd Experiment as another. Which things I ſay not, as if I ſcrupl'd to make uſe of the induſtrious Glauber's or any other mans Experiments, eſpecially when I borrow not with them any of the Doctrines I build on them; but becauſe ſince I neither did nor could take any notice of Glauber's Book in mine, I judg'd it requiſite to ſay ſomething to prevent my being thought to have unthankfully taken one of the chief Paſſages of my Diſcourſe from a Book to which I was utterly a ſtranger.

The Reaſons of my thus conſenting to publiſh the following Hiſtory of Fluidity and Firmneſs, without the reſt of thoſe Annotations which I writ upon the ſame Eſſay touching Salt-Petre, are partly, that theſe are my receneſt Compoſures of this Nature, (having been writen but the laſt year ſave one) and were ſet down when I allow'd my ſelf to be leſs unacquainted with Writers addicted to the modern Philoſophy; partly alſo, becauſe the conſiderableneſs of the Subject invited me to make theſe Annotations much more copious, and ſomewhat leſs unaccurate, than my Notes upon almoſt any other part of the Eſſay; & partly, (and indeed principally) becauſe mention being ſometimes made of this Hiſtory in my freſhly-publiſh'd Phyſico-Mechanical Experiments, both the Printer, and ſome Learned Gentlemen who were pleas'd to think that Book not unworthy the Tranſlating, have ſollicited me to let this Treatiſe be annex'd

annexed to the ſeveral Verſions they are about of that Pneumatical piece, and to the Engliſh Edition of the three fore-going Diſcourſes, which the Printer fears would, without the company of theſe or ſome others, make but too thin a Book.

And I thought fit to premiſe to this Hiſtory, the Eſſay concerning Salt-Petre, *not only becauſe it might appear very improper to publiſh Annotations without the Text it ſelf whereunto they relate; but indeed becauſe I find that there are ſtill many Learned Men, of the ſame diſpoſition with thoſe I have mention'd in the beginning of this Preface; whence I am invited to divulge this Eſſay by the ſame Conſiderations that at firſt induc'd me to write it. Eſpecially ſince I remember not that among the new Philoſophers I have met with any one Experiment that does ſo fairly and ſenſibly accommodate ſo many of their Opinions. And indeed I freely confeſs, that I ſhall think my ſelf to have done no uſeleſs ſervice to the Common-wealth of Learning, if I prove ſo fortunate, as by theſe, or any other Writings of mine to the like purpoſe, to beget a good underſtanding betwixt the Chymiſts and the Mechanical Philoſophers, who have hitherto been too little acquainted with one anothers Learning: There being to this very day a great and almoſt general Miſ-underſtanding betwixt the Corpuſcular Philoſophers and the Chymiſts; moſt of Thoſe (on the one hand) looking upon the* Spagyriſts *as a company of meer and irrational Operators, whoſe Experiments may indeed be ſerviceable to Apothecaries, and perhaps to Phyſicians, but are uſeleſs to a Philoſopher that aims at curing no diſeaſe but that of Ignorance; and moſt of the* Spagyriſts (*on the other hand*) *looking upon the* Corpuſcularians (*if I may ſo call them*) *as a ſort of empty and extravagant Speculators, who pretend to explicate the great Book of Nature, without having ſo much as look'd upon the chiefeſt and the difficulteſt part of it, namely the Phænomena that Their Art has added to the former Edition of this vaſt and obſcure Volume. But that ſome of the principal of the Hermetick Opinions may be more handſomely*

accom-

accommodated by the notions of the Phœnician Hypotheſes, than by the common Philoſophy of Elements and ſubſtantial forms, (which yet their Writers ſo frequently allude to and otherwiſe employ) may appear from hence, that whereas the Schools generally declare the tranſmutation of one Species into another, and particularly that of baſer metals into Gold, to be againſt Nature, and Phyſically impoſſible; the Corpuſcular Doctrine rejecting the ſubſtantial forms of the Schools, and making Bodies to differ but in the Magnitude, Figure, Motion or Reſt, and Situation of their component particles, which may be almoſt infinitely varied, ſeems much more favourable to the Chymical Doctrine of the poſſibility of working wonderful changes, and even tranſmutations in mixt Bodies. And on the other ſide, there are ſcarce any Experiments that may better accommodate the Phœnician principles, than thoſe that may be borrowed from the Laboratories of Chymiſts. For firſt, Chymiſtry enabling us to depurate Bodies, and in ſome meaſure to analyſe them, and take aſunder their Heterogeneous parts, in many Chymical Experiments we may better than in others know what manner of Bodies we employ, Art having made them more ſimple or uncompounded than Nature alone is wont to preſent them us. And next, many Chymical operations being performed in cloſe, and yet in tranſparent veſſels, we may better know what concurs to the effects produced, becauſe adventitious Bodies (or at leaſt all groſſer ones) are kept from intruding upon thoſe whoſe Operations we have a mind to conſider. And laſtly, the Bodies employ'd by the Chymiſts being for the moſt part active ones, the progreſs of Nature in an Experiment, and the ſeries of ſucceſſive alterations through which the matter paſſes from firſt to laſt, is wont to be made more nimbly, and conſequently becomes the more eaſie to be taken notice of and comprehended. So that all this conſidered, I hope it may conduce to the Advancement of Natural Philoſophy, *if, as I ſaid, I be ſo happy as, by any endeavours of mine, to poſſeſs both Chymiſts*

and

and Corpuſcularians of the advantages that may redound to each Party by the Confederacy I am mediating between them, and excite them both to enquire more into one anothers Philoſophy, by manifeſting, that as many Chymical Experiments may be happily explicated by Corpuſcularian Notions, ſo many of the Corpuſcularian Notions may be commodiouſly either illuſtrated or confirmed by Chymical Experiments.

A

PHYSICO-CHYMICAL ESSAY,

CONTAINING

An Experiment with ſome Conſidera-rations touching the differing Parts and Redintegration of

SALT-PETRE.

SECTION I.

SALT-PETRE, *Pyrophilus*, though in that form wherein it is ſold in Shops, it be no very obvious Concrete; yet either in its rudiments, or under ſeveral diſguiſes, it is to be found in ſo great a number of Compound Bodies, Vegetable, Animal, and even Mineral, that it ſeems to us to be not only one of the moſt Catholick of Salts, but ſo conſiderable an Ingredient of many ſublunary Concretes, that we may juſtly ſuppoſe it may well deſerve our ſerious enquiries, ſince the knowledge of

it may be very conducive to the diſcovery of the Nature of ſeveral other Bodies, and to the improvement of divers parts of Natural Philoſophy.

SECT. II.

But not having at preſent much leiſure allow'd me by ſeveral avocations to make accurate Enquiries into the nature of Salt-Petre in general; and, which is more conſiderable, being not yet furniſh'd with a competent number of Experiments requiſite to ſuch a purpoſe; I muſt content my ſelf for this time to tender you ſome aſſiſtance towards the diſcovery of how differing Subſtances may be obtain'd from Nitre, and compound it again, by preſenting you ſome Reflections on an Experiment, which my deſire to haſten to another Subject obliges me to ſet down nakedly, as I firſt try'd it, by way of Narrative.

SECT. III.

We took then common Nitre (as we bought it at the Druggiſts) and, having by the uſual way of Solution, Filtration, and Coagulation, reduc'd it into Cryſtals, we put four ounces of this purifi'd Nitre into a ſtrong new Crucible *in which (the Veſſel being firſt well neal'd to prevent cracking, and cover'd to prevent the falling in of any

** If it be here demanded, Why the Experiment was not made with a greater quantity of Salt-peter? we may anſwer, That the mention'd Quantity was moſt proportionate to the beſt Crucible we then had. And if it be further ask'd, Whether it were not the better way of obtaining the ſeveral ſubſtances ſeparable from Nitre, to diſtil it in cloſe Veſſels without addition of any foreign Body? we ſhall reply only by repreſenting, That the propos'd way is not ſo practicable as one would imagine: for not acquieſcing in the common practice of Chymiſts, who are wont to mingle with the Salt-Petre they diſtil three or four times its weight of Brick, Earth, or ſome other Additament, which (eſpecially in ſo great a proportion) may much alter the Nature of the fix'd Salt remaining behind with it; we have had the curioſity to try more than once, whether we could diſtil Salt-Petre per ſe in glaſs Retorts, and found, that though to avoid giving too ſtrong a fire, we once (at leaſt) plac'd the Retort only in a pan-ful of Sand; yet when the heat was grown ſtrong enough to melt the Salt, it crack'd the Retort, and did partly run out at the crack; only we obtain'd ſome ſmall quantity of a Liquor, which by its ſowreneſs and operation taught us what we might have expected of the reſt of the volatile part of the Nitre, in caſe the Veſſel would have held till it had paſſed over into the Receiver.*

thing

thing that might unſeaſonably kindle the Petre) we melted it into a limpid Liquor, and whilſt it was in fuſion, caſt into it a ſmall live Coal, which preſently kindled it, and made it boil and hiſs, and flaſh for a pretty while: after which we caſt in another glowing Coal, which made it fulminate afreſh; and after that we caſt in a third and a fourth, and ſo continu'd the operation till the Nitre would neither fulminate nor be kindled any more: after which we continu'd to keep it in a ſtrong fire for above a quarter of an hour, that if any volatile part ſhould yet remain, it might be forced off.

SECT. IV.

Then taking out the Crucible, and breaking it whilſt it was hot, we took out, as carefully as we could, the remaining fix'd Nitre before it had imbib'd much of the moiſture of the air, and dividing it into two equal parts, we diſſolved one of thoſe portions in as much fair water as would juſt ſuffice for the ſolution of it, and then drop'd on it Spirit of Salt-petre till the ebullition occaſion'd by the mutual action of thoſe contrary Liquors did perfectly ceaſe; and forthwith Filtrating this mixture, we expos'd it in a new open Vial to the air in a window; and returning to the other portion of fix'd Nitre, which we had ſet apart and not diſſolv'd, we drop'd on that likewiſe of the ſame Spirit till the hiſſing and ebullition were altogether ceas'd, and then we expos'd this mixture alſo in an open glaſs Jar to the air in the ſame window with the former.

SECT. V.

The event of theſe Trials was, that the mixture wherein fair water was employ'd, did in a few hours faſten to the lower part of the ſides of the Glaſs wherein it was put, ſome ſaline particles, which ſeem'd by their form (and partly too by their ſhooting about the lower parts of the Veſſel) to be Salt-petre; amongſt whoſe little Cryſtals nevertheleſs there appear'd to ſwim very little grains

(much ſmaller than Muſtard-ſeeds) of ſome other kind of Salt, environ'd with a downy matter, not unlike that which is oftentimes to be obſerv'd in Roſe-water, and ſeveral other diſtill'd Waters when they begin to decay. The Cryſtals were the next day taken out, being by that time grown ſomewhat greater, and more numerous, and diſclos'd themſelves, upon tryal, to be indeed Nitrous, as well by their manner of burning, as their ſhape. Concerning the latter of which, ſince learned Modern Writers have miſ-repreſented it, ſome making Nitre to be Cylindrical, and others of a figure leſs approaching to the true one; I think my ſelf oblig'd in this place to obſerve to you by the way, that having purpoſely conſider'd ſome large Cryſtals of refin'd and unanalyz'd Nitre, the figure being in ſuch beſt diſcern'd, they appear'd to have each of them ſix flat ſides (not always of equal breadth in reſpect of one another) whereof any two that were oppoſite were commonly parallel. But to return to our augmented Cryſtals of Nitre: what the other matter that adher'd to them was, there was ſo very little of it, that we could not well diſcern, though we then ſuſpected it to proceed from the want of a juſt or exact proportion betwixt the Volatile and fix'd parts of the Nitre that were to be re-united.

SECT. VI.

The remaining Liquor being pour'd into an open glaſs Jar, and left in the ſame window, continued five or ſix days without manifeſting any conſiderable alteration; but at the end of that time there began to appear in it very fine cryſtalline ſtyriæ of Petre, which grew more and more numerous during a fortnight longer; at which time, being wearied with attending the ſo ſlow conſumption of the Liquor, we pour'd it from the Cryſtals, and ſet it in a digeſting Furnace to evaporate more nimbly.

SECT. VII.

The other mixture wherein no water was employ'd did preſently,

preſently, for a great part of it, ſubſide in the form of Salt; over which there ſwam a little liquor which alſo ſeem'd to keep the ſubſiding particles of Salt from congealing into one coherent maſs, or ſo much as greater lumps: and a part of this drenched Salt being taken out and permitted to dry in the Air, did not appear very regularly figur'd, but yet ſeem'd here and there to recede very little from the ſhape of Salt-Petre, and being caſt on a quick coal it burned partly after a manner not peculiar (that we have obſerv'd) to any diſtinct kind of Salt; and yet it partly ſeem'd to imitate the flaſhing way of deflagration proper to Nitre. The remaining part of this Salt, together with the Liquor ſwimming upon it, we kept for about a month in the open air, without diſcerning any obſervable change in the Liquor till towards the latter end of that time, and then we found it partly coagulated into ſmall ſaline maſſes, whoſe figure we were not able to diſcern; and therefore diſſolving the whole mixture in a little fair water, and filtrating it, we found (after evaporation in a digeſting Furnace) about one half of the Salt ſhot into fine ſmall Icicles of the ſhape of Cryſtals of Petre, but ſomewhat differing from them in taſte upon their firſt being put upon the Tongue; but upon a live coal they burned not unlike Petre. And the remaining half of this diſſolution, being ſomewhat haſtily preſſed to exhale, let fall its Salt in a figure which we could not reduce either to that of Salt-Petre, or of any other determinate kind of Salt.

For the clear comprehending of this Experiment, you may be pleas'd, *Pyrophilus*, to take notice,

SECT. VIII.

1. That a new coal is not to be caſt on the Nitre till the detonation occaſion'd by the former be either quite or almoſt altogether ended; unleſs it chance that the puffing matter do blow the coal too ſoon out of the Crucible, (which often enough happens towards the end of the operation:)

ration:) which ſeems to happen chiefly becauſe the firſt part of the Nitre growing to be predominant, the inflamable and halituous particles cannot break through the matter, now grown more ſtiff, but by ſuch impetuous eruptions as make them oftentimes toſſe back the coals as ſoon as ever they are caſt into the Crucible: and in this way of proceeding we have been forced to ſpend much more time, than the opinion of the ready deflagrability (if I may ſo ſpeak) of Salt-Petre did beforehand permit us to imagine.

SECT. IX.

2. That we diſcern'd by our Scales, that the weight of the Spirit of Nitre requiſite to be drop'd on, till all the ebullition made betwixt that Liquor and the Solution of fix'd Nitre were ceas'd, did not amount to ſo great a weight as the Salt-Petre loſt in its detonation, and yet fell not much ſhort of it.

SECT. X.

3. That the fix'd Nitre this way made, differ'd but very little from vulgar Salt of Tartar in its lixiviate taſte, in its aptneſſe to attract the air, or to relent by the moiſture of it, and in its other more obvious qualities; onely whereas Salt of Tartar is wont to be white (which nevertheleſs being flux'd has been by others, as us, obſerv'd to become of a kind of sky-colour) this fix'd Nitre was of a deep colour betwixt blew and green: which colour upon the affuſion of the Spirit of Nitre vaniſh'd; whereas otherwiſe (to obſerve that to you upon the by) ſome ſort of calcin'd Nitre will ſo obſtinately retain that colour, that I keep by me a blewiſh green Liquor made of fix'd Petre, I know not how oftentimes ſucceſſively reſolv'd *per deliquium* and coagulated again, till it would no longer be reduc'd to a dry Salt, but to an unctuous body eaſily flowing in heat like Wax: my deſign in which trial it were here ſomewhat improper to inſiſt on.

That

That our friends might not be obliged to wait so long for the Redintegration of Nitre, but might see the Experiment made in as little time as is possible, we devis'd a more expeditious way of uniting the divorc'd parts of our Salt, and it was only by suffering such fix'd Nitre as is mention'd by our Author to run per Deliquium *into a Liquor, which being separated from its fæces by filtration through cap-paper was very clear and limpid: For, when we had a mind to shew the Experiment, we did onely upon this Liquor drop some Spirit of Nitre, and that, after such a noise, sparkling and effervescence, as our Author speaks of, (all which hastily vanished) did immediately associate it self with a competent proportion of the fix'd Salt swimming in the Solution, and therewith fall down in little Ice-icles of a Nitrous shape and nature; and when we pleased to continue the affusion of the acid Spirit, this emergency of Salt-Petre would be observable from time to time, till either all, or almost all the fix'd Salt had united it self with the other. And these litle Ice-icles being dried in lumps, did as well upon the tongue and upon a quick coal, as they had done to the Eye, disclose themselves to be so truly Nitrous, that our friends were not wont without some wonder, as well as much pleasure, to behold Salt-Petre thus suddenly generated in lesse than a minute of an hour. These small Ice-icles being in sufficient plenty dissolv'd in fair water, we did for trials sake reduce by Congelation to fairer Crystals.*

But though this be the perfectest and readiest way of reproducing Nitre, yet because it often requires, especially in dry weather, a long time to reduce fix'd Nitre per Deliquium *into a Liquor, we have sometimes substituted the following way. We dissolv'd in fair water as much fix'd Nitre as we could, and filtrating the Solution through Cap-paper we satiated it with Spirit of Nitre, after the manner above describ'd, and then setting it to evaporate very slowly, and afterwards suffering it to cool, we obtain'd within some hours after the first mixture of the Liquors, store of fine little Crystalls of Petre, which shot in the Liquor; the remaining part of which being evaporated afforded more of them. And though the evaporation and Crystallization cost us divers hours, yet it seem'd that the Salt-Petre was produc'd presently upon the ceasing of the conflict betwixt the two Liquors. For the mixture before evaporation tasted very like a Solution of common Nitre, and the little drops that upon the effervescence skip'd out of the Glasse, and fell back upon the sides of it, did there many of them presently coagulate into little grains of Nitrous Salt.*

SECT. XI.

And becauſe, *Pyrophilus*, it may be ſuſpected, that the Salt-Petre mention'd to have been produc'd by the reunion of the volatile and fix'd part of that Concrete, may have been onely ſome aſſociated particles of Salt-Petre, that by lurking undiſcernedly in the fix'd Nitre had eſcap'd the analizing violence of the fire, and by the affuſion of fair water were ſet at liberty to aſſemble together, and thereby diſcloſe themſelves in their true ſhape.

To remove this ſcruple, and to let you ſee that much

of

the enſuing Diſcourſe will not need your ſuppoſing, that the Experiment of the Redintegration of Petre was accurately made, and did accordingly ſucceed; I muſt here annex, that though by divers other Chymical Experiments which I have had occaſion to make with Salt-Petre, I ſomtimes diſcover'd, that now and then ſome undiſcern'd particles of the Salt-Petre may poſſibly eſcape our diligence when we make fix'd Nitre; yet thoſe particles are too few to amount to ſuch Cryſtals of Petre, as the affuſion of the acid Spirit upon the lixiviate Salt are capable of affording: And that we have, to ſatisfie our ſelves farther in this particular, purpoſely ſatiated, according to the former manner, a Solution of common Pot-aſhes, bought of them that are wont to ſell it in Shops, (who are not ſo fooliſhly knaviſh as to adulterate them with Salt-petre, which is much dearer than pot-aſhes) and filtrating the Solution from its copious *fæces*, found after evaporation, in the remaining Liquor, within about two or three days, and ſometimes much ſooner, pretty ſtore of Cryſtalline Salt in a Nitrous figure, which though at firſt it taſted ſomewhat corroſively, (perhaps becauſe the proportion betwixt the Nitrous Spirit and the Pot-aſhes was not duely obſerv'd) yet after it had a while remain'd upon the tongue, the taſte of it much emulated that of Salt-Petre; and part of it being caſt upon a live coal, did by its blew and halituous flame diſcover it ſelf to be of the nature of that Salt. To which we may add, that we likewiſe tried the experiment with *Aqua fortis* and Salt of Tartar, and thereby produc'd Salt-Petre, though but in ſmall quantity, and a long time. And thoſe two additional Experiments I the rather mention, becauſe many of the enſuing Reflections may be juſtifi'd by them, although the main Experiment made on Salt-petre alone ſhould in divers pariculars be ſuppos'd (for we have us'd our endeavours that it may not be found,) to have been miſtaken.

SECT.

SECT. XII.

The Reflections which may be made on this Experiment are more than I have either the skill or leisure to prosecute, and therefore I shall content my self to present you very succinctly with a few of those that do the most readily occur to my present thoughts.

And first, this Experiment seems to afford us an instance, by which we may discern that Motion, Figure, and Disposition of parts, and such like primary and mechanical Affections (if I may so call them) of Matter, may suffice to produce those more secondary Affections of Bodies which are wont to be called Sensible Qualities.

SECT. XIII.

And to begin with the Tangible Qualities, as Heat and Cold; it is commonly held, that Salt-Petre is in operation a Cold Body, if not one of the coldest in the world; and accordingly Physicians and Chymists are wont to give it in Fevers to allay the inward exæstuations of the blood and humors: and that profound Naturalist the Lord *Verulam* highly commends a little of it, and did for many years himself make use of it, to condense the Spirits. But what-ever it be in inward operation, certainly to the outward sense it appears very cold: And yet the parts of this so cold Body (its Spirit and Alkali, by the latter of which Chymists are wont to mean any fix'd Salt produced by burning) put together, do immediately agitate each other with great vehemency; and did in our Experiment produce such an heat, that I could scarcely endure to hold in my hand the Vial, wherein much lesse than an ounce of each was mix'd, though but leisurely and almost by drops: as if Heat were nothing but a various and nimble motion of the minute particles of Bodies. For in our Experiment, as long as that confus'd agitation lasted, so long the heat endur'd, and with that agitation it

the enſuing Diſcourſe will not need your ſuppoſing, that the Experiment of the Redintegration of Petre was accurately made, and did accordingly ſucceed; I muſt here annex, that though by divers other Chymical Experiments which I have had occaſion to make with Salt-Petre, I ſomtimes diſcover'd, that now and then ſome undiſcern'd particles of the Salt-Petre may poſſibly eſcape our diligence when we make fix'd Nitre; yet thoſe particles are too few to amount to ſuch Cryſtals of Petre, as the affuſion of the acid Spirit upon the lixiviate Salt are capable of affording: And that we have, to ſatisfie our ſelves farther in this particular, purpoſely ſatiated, according to the former manner, a Solution of common Pot-aſhes, bought of them that are wont to ſell it in Shops, (who are not ſo fooliſhly knaviſh as to adulterate them with Salt-petre, which is much dearer than pot-aſhes) and filtrating the Solution from its copious *fæces*, found after evaporation, in the remaining Liquor, within about two or three days, and ſometimes much ſooner, pretty ſtore of Cryſtalline Salt in a Nitrous figure, which though at firſt it taſted ſomewhat corroſively, (perhaps becauſe the proportion betwixt the Nitrous Spirit and the Pot-aſhes was not duely obſerv'd) yet after it had a while remain'd upon the tongue, the taſte of it much emulated that of Salt-Petre; and part of it being caſt upon a live coal, did by its blew and halituous flame diſcover it ſelf to be of the nature of that Salt. To which we may add, that we likewiſe tried the experiment with *Aqua fortis* and Salt of Tartar, and thereby produc'd Salt-Petre, though but in ſmall quantity, and a long time. And thoſe two additional Experiments I the rather mention, becauſe many of the enſuing Reflections may be juſtifi'd by them, although the main Experiment made on Salt-petre alone ſhould in divers pariculars be ſuppos'd (for we have us'd our endeavours that it may not be found,) to have been miſtaken.

SECT.

SECT. XII.

The Reflections which may be made on this Experiment are more than I have either the skill or leisure to prosecute, and therefore I shall content my self to present you very succinctly with a few of those that do the most readily occur to my present thoughts.

And first, this Experiment seems to afford us an instance, by which we may discern that Motion, Figure, and Disposition of parts, and such like primary and mechanical Affections (if I may so call them) of Matter, may suffice to produce those more secondary Affections of Bodies which are wont to be called Sensible Qualities.

SECT. XIII.

And to begin with the Tangible Qualities, as Heat and Cold; it is commonly held, that Salt-Petre is in operation a Cold Body, if not one of the coldest in the world; and accordingly Physicians and Chymists are wont to give it in Fevers to allay the inward exæstuations of the blood and humors: and that profound Naturalist the Lord *Verulam* highly commends a little of it, and did for many years himself make use of it, to condense the Spirits. But what-ever it be in inward operation, certainly to the outward sense it appears very cold: And yet the parts of this so cold Body (its Spirit and Alkali, by the latter of which Chymists are wont to mean any fix'd Salt produced by burning) put together, do immediately agitate each other with great vehemency; and did in our Experiment produce such an heat, that I could scarcely endure to hold in my hand the Vial, wherein much lesse than an ounce of each was mix'd, though but leisurely and almost by drops: as if Heat were nothing but a various and nimble motion of the minute particles of Bodies. For in our Experiment, as long as that confus'd agitation lasted, so long the heat endur'd, and with that agitation it

encreas'd and abated; and at length when the motion ceas'd, the heat also vanish'd.

SECT. XIV.

Upon the mixture of the two fore-mention'd Liquors there was also produc'd a very audible sound, not unlike the hissing produc'd by the quenching of a live coal in water; and this hissing was, as that other is wont to be, accompanied with an effervescence and boyling up of the Liquor, with store of bubbles, till it was ready to run over the Vessel. This sound seem'd to proced from the nimble and smart percussions of the ambient air, made by the swift and irregular motions of the particles of the Liquors: And such a kind of sound, but much lowder, was produc'd by the impetuous eruptions of the halituous flames of the Salt-Petre upon the casting of a live coal upon it. What interest such a smartnesse in striking the air hath in the production of Sound, may in some measure appear by the motion of the bullet, and that of a switch or other wand, which produce no sound if they do but slowly passe through the air; whereas if the one do smartly strike the air, and the other be shot out of a Gun, the celerity of their percussions on the air puts it into an undulating motion, which reaching the Ear, produces an audible noise even at a good distance from the body, whose swift passage causes those nimble vibrations in the air, as we may elsewhere have occasion to declare. And that in the sound observable in our Experiment, the contiguous air receives many strokes from the particles of the Liquor, seems probable, by the sudden and eager tumultuation of the parts of the Liquors: and by this, that the noise encreas'd and decay'd proportionably to the ebullition of the Liquors, and ceas'd altogether as soon as the saline particles floating in them had by their conflict tir'd themselves into quietnesse. And it is to be observ'd, that the noise ended long before the heat. To the latter of which such an

intestine

inteſtine tumult of the parts of many bodies is ſufficient, as is yet uncapable to produce a ſound. As we ſee in Amber or good hard Wax heated by rubbing; and in many Liquors which retain a conſiderable degree of heat a good while after the expiration of the noiſe they made in boyling.

SECT. XV.

We mention'd alſo, that our fix'd Petre was of a blewiſh green colour, which upon the affuſion of the acid Spirit ſuddenly vaniſh'd. That diſpoſition of parts, whereby the light reflected to the eye, was ſo modify'd as to produce that colour being now alter'd. And the like change we have ſometimes obſerv'd to be producible in fix'd Nitre, by the bare leaving it a while in the moiſt air. To which I muſt add, that in ſome ſuch kind of Experiments I have obſerv'd the copious fumes, ariſing from the mixture, to make the unfill'd part of the Glaſſe look of a reddiſh colour; which is not more odd than that which we have lately had opportunity to obſerve in Soot, which though it be ſo black it ſelf, and reſult from the coalition of dark exhalations, yet preſs'd with a ſtrong fire, has fill'd our Receivers with fumes white enough to make them look as if they were repleniſh'd with milk. And we have ſometimes alſo taken great pleaſure to behold the variety of Colours which may be now and then diſcern'd in the ſublimate, made by gradually ſubliming in an Urinal a mixture of equal parts of only white Sal-Armoniack and black Antimony. But to wander no longer far from our preſent Experiment, give me leave to inform you, that a while ſince attempting to make Salt of Tartar, reſolv'd in a little fair water, an ingredient of Salt-Petre by the addition of good *Aqua fortis*, the union of theſe two Liquors produced a deep green colour, which not only was diffus'd quite through the mixture, but alſo appear'd to reſide peculiarly in certain particles of it. For having for trial ſake filtred it through Cap-paper, there remained in the Filtre

encreas'd and abated; and at length when the motion ceas'd, the heat also vanish'd.

SECT. XIV.

Upon the mixture of the two fore-mention'd Liquors there was also produc'd a very audible sound, not unlike the hissing produc'd by the quenching of a live coal in water; and this hissing was, as that other is wont to be, accompanied with an effervescence and boyling up of the Liquor, with store of bubbles, till it was ready to run over the Vessel. This sound seem'd to proced from the nimble and smart percussions of the ambient air, made by the swift and irregular motions of the particles of the Liquors: And such a kind of sound, but much lowder, was produc'd by the impetuous eruptions of the halituous flames of the Salt-Petre upon the casting of a live coal upon it. What interest such a smartnesse in striking the air hath in the production of Sound, may in some measure appear by the motion of the bullet, and that of a switch or orher wand, which produce no sound if they do but slowly passe through the air; whereas if the one do smartly strike the air, and the other be shot out of a Gun, the celerity of their percussions on the air puts it into an undulating motion, which reaching the Ear, produces an audible noise even at a good distance from the body, whose swift passage causes those nimble vibrations in the air, as we may elsewhere have occasion to declare. And that in the sound observable in our Experiment, the contiguous air receives many strokes from the particles of the Liquor, seems probable, by the sudden and eager tumultuation of the parts of the Liquors: and by this, that the noise encreas'd and decay'd proportionably to the ebullition of the Liquors, and ceas'd altogether as soon as the saline particles floating in them had by their conflict tir'd themselves into quietnesse. And it is to be observ'd, that the noise ended long before the heat. To the latter of which such an

intestine

inteſtine tumult of the parts of many bodies is ſufficient, as is yet uncapable to produce a ſound. As we ſee in Amber or good hard Wax heated by rubbing; and in many Liquors which retain a conſiderable degree of heat a good while after the expiration of the noiſe they made in boyling.

SECT. XV.

We mention'd alſo, that our fix'd Petre was of a blewiſh green colour, which upon the affuſion of the acid Spirit ſuddenly vaniſh'd. That diſpoſition of parts, whereby the light reflected to the eye, was ſo modify'd as to produce that colour being now alter'd. And the like change we have ſometimes obſerv'd to be producible in fix'd Nitre, by the bare leaving it a while in the moiſt air. To which I muſt add, that in ſome ſuch kind of Experiments I have obſerv'd the copious fumes, ariſing from the mixture, to make the unfill'd part of the Glaſſe look of a reddiſh colour; which is not more odd than that which we have lately had opportunity to obſerve in Soot, which though it be ſo black it ſelf, and reſult from the coalition of dark exhalations, yet preſs'd with a ſtrong fire, has fill'd our Receivers with fumes white enough to make them look as if they were repleniſh'd with milk. And we have ſometimes alſo taken great pleaſure to behold the variety of Colours which may be now and then diſcern'd in the ſublimate, made by gradually ſubliming in an Urinal a mixture of equal parts of only white Sal-Armoniack and black Antimony. But to wander no longer far from our preſent Experiment, give me leave to inform you, that a while ſince attempting to make Salt of Tartar, reſolv'd in a little fair water, an ingredient of Salt-Petre by the addition of good *Aqua fortis*, the union of theſe two Liquors produced a deep green colour, which not only was diffus'd quite through the mixture, but alſo appear'd to reſide peculiarly in certain particles of it. For having for trial ſake filtred it through Cap-paper, there remained in the Filtre

a powder of a very deep and lovely colour, but in ſo little quantity, that we could not attempt any Experiment upon it to make it confeſs its nature. But this circumſtance is not to be omitted, that the Salt of Tartar that was then employ'd was extraordinay pure, having been by a peculiar art (elſewhere to be taught you) brought, without any addition, into fair-figur'd Cryſtals almoſt like lumps of white Sugar-candy. To which I muſt add, that the ſame *Aqua fortis* with a Solution of other pure Salt of Tartar, did likewiſe produce a colour much reſembling the former, but much fainter. And it is farther to be taken notice of on this Subject, partly, that Nitre it ſelf, although it ſeem to have nothing of kin to Redneſſe, doth in diſtillation yield blood-red fumes (fondly call'd by ſome Chymiſts the blood of the *Salamander*) which fall again into a Liquor that has nothing of red in it; and partly, that the fix'd Nitre, that did before appear opacous, by a new diſpoſition of its parts conjoyn'd with thoſe of its reimbib'd Spirit, becomes again ſomewhat Diaphanous and Cryſtalline as it was at firſt.

SECT. XVI.

Upon the mixture of theſe two Liquors there alſo obtrudes it ſelf upon the Senſe a very ſtrong and offenſive ſmell, proceeding from the Spirit of Petre; which perhaps occaſion'd ſome Chymiſts to call a Menſtruum (wherein that Nitrous ſpirit and ſmell is predominant) the *Stygian* water. But though the Nitrous Spirit have a very ſtrong and unwelcome odour of it ſelf, yet is it made much more offenſive by being pour'd on its own fix'd Salt; for upon their conflict, the matter, being vehemently agitated, doth more copiouſly emit ſuch ſtinking exhalations than before, and ſendeth forth fumes manifeſtly diſcernable as well to the Eye as Noſtrils. The odour of the fix'd Nitre is very languid; but that which it diſcovers being diſſolv'd in a little hot water is altogether differing from

from the ſtink of the other, being of kin to that of other alkalizate Salts. And yet the Salt-Petre from which ſuch differingly-ſented bodies ſpring, and which may again emerge from the coalition of them, has not been obſerv'd, as I remember, to have any ſmell at all.

SECT. XVII.

The taſtes of theſe two bodies are as differing as any of their other qualities: for the Spirit is exceedingly acid, and may be call'd a ſtrong and ſour *Acetum Minerale*; whereas the fixt Nitre has as ſtrong a taſte of Salt of Tartar as the Spirit has of diſtill'd Vinegar: and yet theſe two bodies, whoſe ſapours are ſo pungent, and ſo differing, do both ſpring from and unite into Salt-Petre, which betrays upon the tongue no heat nor corroſiveneſſe at all, but coldneſſe mixt with a ſomewhat languid reliſh retaining to bitterneſſe. And though we muſt not conceal from you, that in our trial the redintegrated Salt-Petre had upon its firſt impreſſion upon the tongue a taſte more ſharp and perforating (if I may ſo ſpeak) than ordinary Nitre; yet that pungency may not improbably be ſuppoſed to have proceeded from ſome Acid particles of the Spirit that were not yet duly incorporated with, but onely looſely adherent to, the more perfectly Nitrous parts, which afterwards diſcover'd it ſelf upon the tongue. And however, the difference betwixt the taſte of this new Salt, and thoſe of the Acid and Alkalizate Salts whereof it conſiſted, and (unqueſtionably) the taſte of theſe compared with that of the crude Petre which was diſſipated into them, were ſufficient to warrant this Reflection.

SECT. XVIII.

Of the other Obſervables preſented us by our Experiment, we muſt, *Pyrophilus*, content our ſelves to mention but a few; our haſte being ſuch that it will not permit us either to enumerate them all, or to inſiſt long on any of them.

SECT.

SECT. XIX.

Secondly then, the propoſed Experiment ſeems to make it ſome what queſtionable, whether or no Inflammability doth ſtrictly in all mixt bodies require a diſtinct Sulphureous ingredient; and whether or no in ſome Concretes it may not reſult from ſuch a contrivance of parts, as that thereby the particles of the Concrete are diſpos'd to be ſet a moving by the adventitious whether Fiery or Calorifick Corpuſcles of another body, in ſuch numbers, and with ſuch celerity, as may put them into that Scheme of matter which we call Flame. How violent an heat may be produc'd upon ſuch an account as this, may in ſome meaſure appear by an Experiment wherein our preſent Theme Salt-petre is the main Agent. For if into a Vial fill'd with good Spirit of Nitre you caſt a piece of Iron, you may perceive that the Liquor, whoſe parts mov'd placidly and uniformly before, manifeſted no heat to the touch, meeting with pores and particles in the Iron capable of very much altering the motion of its parts, (and perhaps alſo that of ſome very ſubtile Intercurrent matter,) thoſe active parts do preſently begin to penetrate, ſever, and ſcatter abroad the particles of the Iron (almoſt as Gunpowder doth the pieces of breaking Granadoes) with ſuch rapidity, and in ſuch plenty and throngs, that being themſelves alſo put into a very ſwift and irregular motion (whence ſoever it proceeds) there is hereby produc'd a heat capable (if the quantity of the Liquor and Metall be great enough) to burn his hand that holds the Veſſel, and perhaps break the Veſſel (if it be not very open) all to pieces; whereas by caſting into the ſame Spirit of Nitre little lumps of Camphire, whoſe particles were indiſpos'd to occaſion the like diſturbance and agitation in the Nitrous Spirit, we obſerv'd the agitation made of the particles of the white Gum to change it onely into a yellowiſh and fluid ſeeming Oyl.

SECT.

SECT. XX.

But not to wander any further, our own experiment informs us, that Salt-Petre (which not onely is inflammable, but burns very fiercely and violently) may be produc'd by the coalition of two bodies, which are neither of them inflammable; the one being a fix'd Salt, that to become such has already suffer'd the loss of all that the fire could deprive it of, and the other being a Spirit abounding with acid particles, which kind of Salts have been observ'd to be more apt to quench than foment fire.

SECT. XXI.

And because we may else-where, God assisting, treat more particularly of the Inflammableness of Bodies, we will now add but a few lines concerning that of Nitre, that this circumstance of it might not escape our observation, namely, that upon casting Salt-Petre on a glowing coal, or upon the casting of a glowing coal into melted Salt-Petre, the Nitre will immediately take fire and flash out into blewish and halituous flames; whereas if the same Nitre be plac'd in a Crucible, though that Crucible be by degreees made glowing hot, and do immediatly with its concave surface in innumerable places touch the particles of Nitre, yet the strange Salt will be thereby melted, but not kindled. The Reason of which Phænomenon I must not now (but may on another Occasion) spend time to enquire after.

SECT. XXII.

It may also, *Pyrophi.* deserve the Enquiry, whence it proceeds that whereas the body of Salt-Petre when committed to Distillation is oftentimes very well dry'd, and consists of Saline parts which are generally accounted to be of a very dry nature, yet the spirits of Petre forc'd by the fire into the Receiver should not, like Sal-Armoniack, and some other bodies distill'd with the like heat and vessels, adhere in the form of Sublimate to the Receiver, but fall into a liquor, which does not, for ought we have seen

ſeen or heard of, either totally or in part coagulate again in the cold, as we have ſeen Spirit of Urine and other volatile liquors (afforded by Animal ſubſtances) often do; and as we have obſerv'd, though rarely, ev'n in the corroſive liquor that is wont to be call'd Butter of Antimony. And the like Enquiry may be made concerning the liquidneſſe of the diſtilled Spirits of decrepitated Salt, calcin'd Vitriol, and divers other bodies, which ſeem to have been deſtitute of moiſture, when committed to Diſtillation.

SECT. XXIII.

But this not being preciſely a Phænomenon of our Experiment, we ſhall not here proſecute it, (though perhaps we elſe where may) but rather obſerve to you, *Pyrophilus*, that whereas good Spirit of Nitre being left in an open veſſel, is wont to ſmoke and waſte it ſelf in an Exhalation ſenſible, eſpecially if it be excited by a little heat, not only in the Noſe but to the Eye; this Fugitive Spirit when it is once re-united to its own fix'd Salt, emits no ſuch ſteam, though kept a good while near a conſiderable fire: which Inſtance may ſomewhat aſſiſt us to make out, that the moſt fugitive parts of Concretes may in ſpight of their natural Mobility be detain'd in bodies by their Union and texture with the more ſluggiſh parts of them, among which thoſe lighter and more active Ingredients may be ſo entangled as to be reſtrain'd from Avolation.

SECT. XXIV.

Another thing worth conſidering in our Experiment is this, that upon the dropping of the acid ſpirit into the Alkalizate liquor, if you place the open-mouthed glaſſe wherein the Experiment is perform'd betwixt the light and your eye, you may plainly diſcern, that the Saline particles of theſe liquors toſſe one another (or are toſſed by ſome brisk inviſible ſubſtance) to the height of divers fingers

fingers breadth up into the air, whence moſt of them fall back into the Veſſel like a thick ſhower of little drops of rain: And it were worth enquiring, whence this ſparkling of the parts of theſe mixt liquors ariſes; and whether the Saline Corpuſcles may be conceiv'd rapidly to move differing ways, and ſo, thwarting each other in their courſes, and rudely juſtling at their Occurſions, ſome of them are forc'd to bound or fly upwards, (almoſt like Ivory balls meeting each other on a Billyard-Table.) And to aſſiſt you in this Enquiry, give me leave to inform you, that the particles thus thrown into the air appear to be moſt of them Saline by this Obſervation; that ſoon after the fall of the fore-mention'd ſhowers, you ſhall find the ſides of the glaſſe wherein the affuſion of the Nitrous ſpirit has been made, all embroidered with little grains of Salt, left there by thoſe wandring drops that fell beſides the liquor.

SECT. XXV.

And let me farther obſerve to you, that there ſeems to be a very nimble agitation in the particles of the Spirit of Nitre, by this, That upon the pouring of *Aqua fortis* (whoſe Active part is little elſe than Spirt of Nitre) upon a Solution of Salt of Tartar in fair water, in which divers ſmall lumps of the Salt remain'd yet undiſſolv'd, we have obſerv'd the acid ſpirit to ſever the particles of the Salt with ſuch impetuoſity, that the numberleſſe little Bubbles produc'd upon their Conflict, and haſtily aſcending in ſwarms from ſome of the little lumps, made them emulate ſo many little, but rapidly riſing, Springs. And to make it yet appear more probable, that there may be ſuch croſſing motions in the parts of theſe liquors, we obſerv'd, that after the two contrary Salts had by their mutual conflict tir'd each other, (or rather had been upon their occurſions faſtned to one another) there would follow no farther ebullition or skipping up and down of little

drops of the liquors, upon the putting in of more Spirit of Nitre, unleſſe there were added likewiſe more of the Alkalizate liquor.

SECT. XXVI.

And, before we paſſe on from this Reflection, it may not be uſeleſſe to take notice of the difference that there may be betwixt thoſe active parts of a body which are of differing Natures, when they are as it were Sheath'd up, or Wedg'd in amongſt others in the texture of a Concrete; and the ſame particles, when (extricated from theſe Impediments) they are ſet at liberty to flock together, and by the exerciſe of their nimble motions diſplay their proper, but formerly clogg'd activity, or acquire a Diſpoſition to be briskly agitated by ſome fine interfluent matter. For though in the entire body of Salt-Petre the Ingredients it conſiſts of, or the differing ſubſtances into which the fire diſſipates it, do ſo mutually implicate and hinder each other, that the Concrete, whilſt ſuch, acts but very languidly; yet when the parts come to be diſlocated, and the halituous and Alkalizate particles are enabled or made to disband from the Concrete, and aſſociate themſelves with thoſe of their own nature, we ſee with how great an activity both the acid Spirit and the fix'd Salt are endow'd.

SECT. XXVII.

And we may yet farther obſerve, that it is not barely an indefinite nimbleneſſe of motion, and activity of the particles of Saline liquors, that enables them to perform each of their particular effects: for to the production of ſome of theſe there ſeems requiſite, beſides perhaps a Modification of their Motion, a determinate Figure of the corpuſcles, anſwerable to that of the pores of the body by them to be diſſolv'd; as Spirit of Nitre corrodes Silver, but not Gold; which nevertheleſſe, its particles aſſociated with thoſe of Sal-Armoniack, and thereby acquiring a new Figure, and perhaps a differing Motion, will readily diſſolve:

diſſolve: and the liquor of fix'd Nitre will for the ſame reaſon, diſſolve ſuch Sulphureous and unctuous bodies as the acid ſpirit will not corrode; nay, and I have carefully obſerv'd, that there may be liquors that will not diſſolve ſome bodies, unleſſe the motion or activity of their particles be allay'd or modify'd by the mixture of fair water, or ſuch unactive vehicles.

SECT. XXVIII.

Another particular which in our Experiment we may take notice of, is, the unwarineſſe of thoſe vulgar Chymiſts who preſume confidently (and indiſcriminately enough) to aſcribe to each of the heterogeneous Ingredients, or (in their language) Principles of a Concrete analys'd by the fire, the virtues and properties (perhaps too in an exalted degree) of the entire body. But though this be an errour of very ill conſequence in reference to divers Chymical preparations of Medicines; yet having elſe-where diſcours'd purpoſely of it, we ſhall here content our ſelves to allege againſt it the inſtances afforded us by the Experiment under conſideration: for in that we may obſerve, that when Salt-Petre is diſtill'd, the volatile liquor and fix'd Salt into which it is reduc'd by the fire, are endowed with properties exceeding different both from each other, and from thoſe of the undiſſipated Concrete: for the Spirit of Nitre is (as we formerly have obſerv'd) a kind of *Acetum Minerale*, and poſſeſſes the Common qualities to be met with in acid ſpirits as ſuch; whereas the fix'd Nitre is of an Alkalizate nature, and participates the qualities belonging generally to lixiviate Salts; and Salt-petre it ſelf is a peculiar ſort of Salt, diſcriminated by diſtinct properties both from thoſe Salts that are eminently acid, as Allum, Vitriol, Sal-gemmæ, &c. and from thoſe that are properly Alkalizate, as Salt of Tartar and Pot-aſhes; and accordingly, we may eaſily obſerve a vaſt diſparity in the effects and operations of theſe three bodies.

For ſeveral, if not all of thoſe mineral Ones which *Aqua fortis* will by corroding diſſolve, the Solution of fix'd Nitre will precipitate; and divers, if not all of thoſe Sulphureous and unctuous bodies which the Solution of fixt Nitre will diſſolve, the acid ſpirit of Petre will precipitate. And we have in a trice re-diſſolved with the Spirit a Solution of Sublimate precipitated with the other liquor: Thus, if into a Scarlet tincture made by an Infuſion of Braſil in fair water, we pour a little Spirit of Nitre, the ſhaken liquor will in a moment change its Redneſſe for a kind of Yellow, which by pouring on it a little of the Solution of fix'd Nitre, may be again graduated into a ſomewhat Sanguine colour, ſometimes paler, and ſomtimes perhaps deeper than the firſt; whereas a Solution of Selt-petre it ſelf pour'd on either of the former tinctures, the Red or the Yellow, has not been by us diſcern'd to have produc'd any ſenſible alteration. And whereas Salt-Petre it ſelf is partly fix'd, and partly volatile, the acid Ingredients of it are altogether volatile, the Alkalizate fix'd. But having elſewhere occaſion to ſpeak to this ſubject, we ſhall now proceed to tell you, that

SECT. XXIX.

It may paſſe for another Obſervable preſented us by our Experiment, that it gives us occaſion to enquire whether the Air doth not contribute ſomething to the artificial production of Salt-Petre, or at leaſt to the figuration of it according to the perfecter ſhape belonging to that kind of Salt: for we formerly obſerv'd, that the Salt which was leiſurely permitted to ſhoot of it ſelf in the liquor expos'd to the open air, did ſhoot into more fair and large Cryſtalline Stiriæ, than thoſe that were gain'd out of the remaining part of the ſame liquor by a more haſty evaporation, though made but in a digeſting Furnace. And we have alſo obſerv'd, that when once we pour'd *Aqua fortis* on a ſtrong Solution of Salt of Tartar, till no further effervescence was diſcernable

diſcernable betwixt them, though the mixture by a ſomewhat quick heat afforded a Salt that ſeem'd to be very Nitrous, yet it would not be brought to ſhoot in ſo fair and conſpicuouſly-figur'd Cryſtals of Petre, till it had been a good while expos'd to the open air: but whether the air its ſelf impregnated with the promiſcuous ſteams of moſt of the bodies of the terreſtrial globe, (and perhaps with ſeminal *effluvia* from ſome of them) do really contribute any thing either to the Production or Figuration of Saltpetre in our Experiment, I dare not yet determine, for two chief Reaſons.

SECT. XXX.

Whereof the firſt is, becauſe the Figuration ſeems not improbably aſcribable, not ſo much to the proper efficiency of the air, as to the conveniency which by quietneſſe, and a competent vehicle to move in, was afforded to the Saline particles, to conform themſelves (or be conform'd by a Concourſe of Agents and Circumſtances) to that figure which is moſt natural to them. For we have obſerv'd already, that the fix'd Nitre which was not diſſolv'd in water, before the affuſion of the acid ſpirit, did not ſhoot into the wonted form of Criſtalls of Petre, but remain'd a kind of Nitrous powder, the acid and Alkalizate Saline particles not having a convenient vehicle to expand themſelves in; but being neceſſitated, for want of room, to make an unſeaſonable and over-haſty coalition, upon which their own weight made them ſubſide in the figures reſulting from their caſual concourſe, and therefore probably differing from thoſe into which the Saline corpuſcles would have been diſpos'd, had they been allowed a competency of vehicle and time.

SECT. XXXI.

The other reaſon of my heſitancy about the uſe of the Air in our experiment, is, that I inconſiderately forgot to try whether part of that Liquor which ſhot into Cryſtals in

in an open-mouth'd glaſs expos'd to the air, would not have done the like, if it had been left quiet as long as the other was, though in a veſſel accurately ſtopp'd:* but whatever the Air hath to do in this experiment, I dare invite you to believe, that it is ſo enrich'd with variety of ſteams from Terreſtrial (not here to determine whether it receive not ſome alſo from Cœleſtial) bodies, that the enquiring into the further uſes of it (for I mean not it's known uſes in Reſpiration, Sayling, Pneumatical Engines, &c.) may very well deſerve your curioſity. To encourage which, I dare at preſent only tell you, that though I cannot yet pretend to much experience in this particular, yet we have known ſuch changes (ſeemingly Chymical) made in ſome Saline Concretes, by the help chiefly of the volatilizing operations of the open air, as very few, ſave thoſe that have attentively conſider'd what *Helmont*, and one or two other Artiſts, have hinted on that ſubject, or have made tryals of that nature themſelves, will be apt to imagine.

* Whether the Air have any great intereſt in the Figuration or in the Reproduction of Nitre, the Author hath ſince examin'd by particular Tryals; but in Veſſels and by ways not to be eaſily deſcrib'd in few words, and therefore the further mention of them is reſerv'd for another Diſcourſe.

SECT. XXXII.

And if upon further and exacter tryal it appears that the whole body of the Salt-Petre, after it's having been ſever'd into very differing parts by diſtillation, may be adequately re-united into Salt-Petre equiponderant to it's firſt ſelf; this Experiment will afford us a noble and (for ought we have hitherto met with) ſingle inſtance to make it probable that that which is commonly called the Form of a Concrete, which gives it it's being and denomination, and from whence all it's qualities are in the vulgar Philoſophy, by I know not what inexplicable wayes, ſuppoſed to flow, may be in ſome bodies but a Modification of the mat-

matter they conſiſt of, whoſe parts by being ſo and ſo diſpoſed in relation to each other, conſtitute ſuch a determinate kind of body, endowed with ſuch and ſuch properties; whereas if the ſame parts were otherwiſe diſpoſed, they would conſtitute other bodies of very differing natures from that of the Concrete whoſe parts they formerly were, and which may again reſult or be produc'd after it's diſſipation and ſeeming deſtruction, by the re-union of the ſame component particles, aſſociated according to their former diſpoſition.

SECT. XXXIII.

The Redintegration (or Reproduction) of an analyz'd body, if it can be accurately and really perform'd, may give much light to many particulars in Philoſophy, and would certainly be very welcome both to the embracers of the Atomical Hypotheſis, and generally to thoſe other Modern Naturaliſts, who aſpire to ſuch Explications of Nature's *Phænomena* as may at leaſt be underſtood: all whom I wiſh, that though men cannot perhaps in all things, yet at leaſt as far as they can, they would accuſtom themſelves to ſpeak and think as Nature does really and ſenſibly appear to work; and not to acquieſce in Notions and Explications of things which, ſtrictly examin'd, are not intelligible.

Wherefore I am about to attempt a Reproduction in Vitriol, Turpentine, and ſome other Concretes, in which it ſeems not unlikely to be performable: and perhaps you may ſee cauſe to think that the Experiment of Salt-Petre, even as we have already made and propoſed it, though it be not an exact and adequate Redintegration, is yet not far from being a real one; the diſſipated parts of the Concrete truly re-uniting into a body of the ſame nature with the former, though not altogether of the ſame bulk.

SECT. XXXIV.

And yet I think it requiſite to repreſent to you, *Pyrophilus*

Philus, that Salt-Petre is a body whoſe parts are not Organical, and which is not ſo much as very compounded; and that therefore bodies that conſiſt of more numerous Ingredients, and much more thoſe whoſe Organical parts require a much more artificial and elaborate diſpoſition or contrivance of their component particles, cannot be ſafely judg'd of, by what is poſſible to be perform'd on a body of ſo ſimple and ſlight a contexture as is Salt-Petre: for we ſee that even wine, though no organical body, nor ſo much as the moſt compounded of inanimate Concretes, when it's ſpirit is, though by the gentleſt diſtillation, drawn from it, will not, by the re-union of it's conſtituent Liquors, be reduc'd to it's priſtine Nature; becauſe the workmanſhip of Nature in the diſpoſition of the parts was too elaborate to be imitable, or repairable by the bare and inartificial appoſition of thoſe divided parts to each other: beſides that in the diſſociating action, even of the gentleſt fire, upon a Concrete, there does perhaps vaniſh, though undiſcernedly, ſome active and fugitive particles, whoſe preſence was requiſite to contain the Concrete under ſuch a determinate form; as we ſee in Wine degenerating into Vinegar, where the change ſeems to proceed from this, that upon the Avolation or (if I may ſo ſpeak) Depreſſion) of ſome ſubtle ſulphureous ſpirits, whoſe Receſſe or degeneration is not to be perceiv'd by any ſenſible diminution of bulk in the Liquor, the remaining parts fall into new leagues or diſpoſitions, and conſtitute an acid Liquor ſomewhat fix'd and Corroſive, and conſequently of qualities very differing from thoſe of the Wine, whoſe ſouring produc'd it: as we more fully declare in our Experiments relating to Fermentation.

SECT. XXXV.

And certainly there is, as we formerly ſaid, ſo artificial a contrivance of particles requiſite to the conſtitution

of

of the Organical parts of living bodies, that it will be ſcarce poſſible for humane Art or Induſtry to imitate ſo as to equal thoſe exquiſite productions of Nature: And therefore I wonder not that the ſtory of the *Phœnix*'s reſurrection out of her own aſhes ſhould by the beſt Naturaliſts be thought a meer fiction. And if that relation, mention'd by the inquiſitive *Kircherus* as an eye-witneſs of the Reproduction (if I may ſo call it) of Shell-fiſhes near the brink of a Lake in the *Sicilian* Promontorie *Peloro*, by the watering of their broken bodies with Salt water in the Spring, be ſtrictly true, it ſeems much more improbable that ſuch changes and viciſſitudes ſhould be bare Redintegrations of the diſſociated parts of ſuch reſtored bodies; than that *, (according to what we elſewhere teach,) they ſhould be New Productions made by ſome ſeminal particles undiſcernedly lurking in ſome part of the deſtroyed body, and afterwards excited and aſſiſted by a Genial and cheriſhing heat ſo to act upon the fit and obſequious matter wherein 'twas harbor'd, as to organize and faſhion that diſpoſed matter according to the exigencies of it's own Nature. For that in ſome bodies the Seminal particles may a while ſurvive the ſeeming deſtruction of life, is not altogether without example, as we elſewhere profeſſedly manifeſt. And in *Kircher*'s ſtory it is to be obſerv'd, that the reſtor'd Animals were but Shell-fiſh; in whoſe ſlimy and viſcous ſubſtance the Spirits and Prolifick parts are probably both more diffuſed and kept from being eaſily diſſipable; to which I know not whether it will be worth while to ſubjoyn, that in ſuch Fiſhes the Mechanical contrivance is but very plain, and as it were ſlight and obvious, in compariſon of the exquiſitely elaborated parts of more perfect Animals.

Lib. 3. de Arte Magnet. Par. 5. Cap. 3.

* In ſome Papers about the origine of Living Creatures ſuppos'd to ſpring of themſelves.

SECT. XXXVI.

The laſt obſervable, *Pyrophilus*, that we ſhall at preſent take notice of in our Experiment, ſhall be this, That it may thereby ſeem probable that ſome Chymical remedies may be too raſhly rejected by Phyſicians, becauſe Oyl or Spirit of Vitriol, *Aqua Regis*, or other Corroſive Liquors, have been employ'd in their preparation: For it is confidently affirm'd by many Phyſicians, and but faintly denied by ſome Chymiſts, that the Corroſive Menſtruums made uſe of in the preparation of remedies, can never be ſo exquiſitely waſh'd off from them, but that ſome of the Salts will adhere to the Medicines, and perniciouſly diſplay their Corroſive Nature in the body of him that takes them. And it is not to be denied, but that many ignorant and venturous Chymiſts do unskilfully (and therefore dangerouſly enough) imploy Corroſives ſometimes without any neceſſity or real advantage to invite them to it, and ſometimes without ſufficiently freeing their Medicines from the corroding Salts, by whoſe aſſiſtance they were prepar'd; for 'tis not always the frequency of ablutions, though with warm water, that will ſuffice to carry off the Salts from ſome bodies; and therefore thoſe great Artiſts, *Helmont* and *Paracelſus*, preſcribe *ſome* things to be dulcifi'd by the abſtraction of the water of whites of Egges, (which though it ſeem inſipid, hath been found a great diſarmer of corroſive Salts) and *others* by the frequent diſtillation of Spirit of Wine, which indeed (not to mention the Balſamick parts it may leave behind) we have obſerv'd to have a faculty of carrying up with it the Saline Particles of Spirit of Vinegar adhering to ſome Chymical remedies. But all this notwithſtanding, *Pyrophilus*, there may be ſeveral bodies (and perhaps more than are commonly taken notice of) which quite alter the nature of the acid Salts employ'd to prepare them, by occaſioning thoſe Salts to degenerate into another nature, upon the very act of corroding, or elſe by

by ſo aſſociating their ownSalts with thoſe of the diſſolving Menſtruum, that from the Coalition of both, there emerges a third body differing in qualities from either. As in our experiment we find that the Spirit of Petre, which is much more ſharp and corroſive than the ſtrongeſt diſtill'd Vinegar, and the fix'd Nitre which is Cauſtick like Salt of Tartar, and may, I ſuppoſe, well ſerve for a Potential Cautery, (as Surgeons ſpeak) do by their mutual action work themſelves into Salt-Petre, which is far enough from having any eminently fretting Quality, and may be ſafely taken inwardly in a much greater Doſe than either of its Ingredients.

SECT. XXXVII.

How much corroſive Salts may dulcifie themſelves by corroding ſome bodies, you may eaſily try by pouring diſtill'd Vinegar or moderate Spirit of Vitriol upon a competent proportion of Corals, or Crabs Eyes, or Pearls, (or, as I ſuppoſe, almoſt any teſtaceous body.) And for my part, though I am very ſhy of imploying corroſive Liquors in the Preparation of Medicines; yet I have lately given a Preparation of refin'd Silver made with *Aqua fortis* it ſelf, or Spirit of Nitre, not onely innocently, but with ſuch ſucceſs, that a couple of Experienc'd Phyſicians themſelves, that were troubled with a ſuperfluity of ſerous humors, ſent to requeſt it of me for their own uſe.

SECT. XXXVIII.

It were therefore worth while, in every Preparation where Corroſive Liquors are wont to be imploy'd, or may ſeem requiſite to be ſo, to conſider the diſtinct nature of the particular bodies to be wrought upon, or conſult Experience *whether* or no the acid Menſtruum do communicate to the Concrete any Particles capable of retaining their fretting Quality after the end of the Operation; or whether or no the Salts do not ſo ſpend and tire themſelves

in the act of corroding, that being as it were ſheath'd, they become unable to corrode any further; or whether or no the Menſtruum do not in the body to be corroded meet with ſome ſuch Saline particles, as may with it conſtitute a new and inoffenſive ſubſtance, as when Spirit of Vinegar, by corroding calcin'd Lead, is turn'd with it into a Salt, not of an acid, but a Saccharine taſte, ſuch as invited Chymiſts to give it the name of Sugar of *Saturn*. In the former of theſe caſes the Medicine may be dangerous, unleſs it be after the Solution or Corroſion ended exquiſitely dulciſi'd from all remainder of the Corroſive Salts. But in the two latter caſes the remedies may in ſpight of the Corroſiveneſs of the Menſtruums employ'd about them, be ſafe and innocent enough; for it matters not much how ſharp and fretting the ſever'd Ingredients of a remedy were, provided the remedy it ſelf reſulting from them be not ſo. And whereas it is objected, that in divers of theſe remedies the Corroſive Salts are not really deſtroy'd, but onely diſguis'd, becauſe by diſtillation it is poſſible to ſeparate from them the Liquors us'd about them as Corroſive as ever: it may eaſily be reply'd, that beſides that in ſeveral Medicines, the Matter of Fact will not hold in divers others; the Objection built on it is much more ſpecious than ſolid, for it very little concerns us to be ſure that out of the Medicines we take or give the violence of fire cannot ſeparate Corroſive Salts; provided we be duely ſatisfi'd, that no ſuch ſeparation can be made by the heat or Juices of a humane body. And therefore, though it have been affirmed, that *Tartarum Vitriolatum* would upon the urgent ſolicitation of a ſtrong fire, part with much of (that moſt fretting liquor upon animal ſubſtances) the Oil of Vitriol, that concurr'd to its Production; yet our beſt and varieſt Phyſicians, not onely Chymiſts but Methodiſts, ſcruple not to give it inwardly in ſeveral conſtitutions and diſtempers. And to end this diſcourſe with the Experiment that began

began it, we clearly ſee that Salt-Petre is frequently and innoxiouſly given inwardly, though the Salt, that makes even *Aqua fortis* ſo Corroſive, be the principal Ingredient of it, and may by diſtillation be driven from it.

SECT. XXXIX.

It would not have been very uneaſie for me, *Pyrophilus*, to have added to divers particulars of the paſt Diſcourſe, Experiments and conſiderations tending to countenance or illuſtrate the Reflections therein ſet down: but in the firſt place, I wanted leiſure to expatiate; in the ſecond place, I was unwilling to anticipate what I have to ſay to you in other Eſſays, eſpecially expecting to have elſewhere occaſion to make mention of Salt-Petre. And beſides all this, I am (to tell you the truth) deſirous to impoſe on you a kind of neceſſity of proſecuting this Experiment, further than when I made it I had opportunity to do. For as I am apt to think it may prove a noble one: ſo I am ſufficiently ſenſible of my having not yet been able to look into the bottom of it; and that very ſenſe of my own ignorance, help'd to keep me from lengthening your trouble in this Eſſay, leſt by ſolemnly endeavouring to countenance my Conjectures, I might be thought Dogmatical in a haſty Scribble, wherein 'tis much more my deſign to awaken and engage your Curioſity, than acquaint you with my opinions. And yet I thought it not amiſs to mention the paſt conſiderations, ſuch as they are, partly becauſe this one inſtance ſeems ſo fairly to accommodate more than one Notion of the intelligible Philoſophy, which ſeems hitherto not to have ſo much as employ'd, much leſs produc'd, any ſtore of Experiments; and partly, becauſe I would have you take notice, that more Obſervables than one may ſometimes be very reaſonably ſought for in a ſingle Experiment. And perhaps too, I was willing by my ſpending a whole Eſſay upon one Experiment, without allowing my ſelf to wander often

often from it, to invite you to think with me, That Experiments ought to be eſtimated by their value, not their number; and that a ſingle Experiment, I ſay not ſuch as that the laſt Eſſay treats of, but in general, ſuch as, it may be, may as well deſerve an entire Treatiſe, as a great many leſs conſiderable ones. As one of thoſe large and orient Pearls that are fit to adorn a Monarchs Crown, may outvalue a very great number of thoſe little (though true) Pearls that are to be bought by the ounce in Goldſmiths and Apothecaries ſhops.

SECT. XL.

Having newly met, *Pyrophilus*, with ſome ſmall Treatiſes freſhly publiſh'd by *Glauberus*, and not having now the leiſure to conſider, or indeed ſo much as to peruſe, much leſs the opportunity to make tryal of divers Particulars, which by turning over the leaves of the book, I find mention'd by him in relation to Salt-Petre, I muſt recommend to you the care of examining the Particulars he delivers; and trying how far ſome of them may ſerve to correct, or to confirm, and how far others may be corrected by what has been in the paſt diſcourſe ſet down concerning Salt-Petre upon Experiments, ſome of whoſe fruits I can yet ſhew you, which were made upon the account of the diviſibleneſſe of Nitre into fix'd and volatile parts, long before the Publication of *Glauber's* Treatiſes.

THE

THE

HISTORY

OF

FLUIDITY

AND

FIRMNESSE.

Begun by *R. B.*

FLUIDITY

AND

PERMANENCE

Being Lectures delivered to a Juvenile Audience

By T. R.

ADVERTISEMENTS

Concerning the following

TREATISE.

THAT the Author in thoſe Animadverſions upon the Eſſay touching Salt-Petre, whereof the enſuing Treatiſe makes a part, might with the more freedom and conveniency adde, alter, and ev'n retract as he ſhould ſee cauſe, he thought fit to write them as if they were made on the Work of another.

The Author hopes, That the Equitable Reader conſidering that the following Particulars touching Fluidity and Firmneſs were firſt written but by way of Annotations upon the beginning of the above-mentioned Eſſay, will excuſe the unaccurateneſs of the Method, as a fault ſcarce evitable on that occaſion. It is alſo hop'd, that if the Reader will remember, that he was told in the Preface to the newly-mention'd Eſſay, that moſt of thoſe whom to gratifie, That Treatiſe and the enſuing Notes on it were written, were addicted to the Epicurean Philoſophy; The Authors Explicating things chiefly according to the Atomical Principles will not be thought ſtrange, nor be lookt upon as a ſure Argument of his being wedded to the particular opinions wherein the Atomiſts differ from other modern Naturaliſts; eſpecially ſince he has on ſome occaſions plainly enough

intimated the contrary, by propoſing, together with the Atomical ways of reſolving a thing, another Explication more agreeable to the Carteſian, or ſome other modern Hypotheſis.

The following Tract was entitl'd a History *of Fluidity and Firmneſs, becauſe indeed the having ſet down Experiments and other matters of fact relating to the Subjects treated of, is the Main, though not the Only thing the Author dares pretend to have done in it. And he ſtiles the Hiſtory as it now comes abroad,* Begun: *Partly, becauſe he would invite abler Pens to contribute their Obſervations towards the compleating of what he is ſenſible he has but begun; and partly becauſe he may hereafter, if God permit, do ſomething of that kind himſelf.*

And laſtly, the Author, though troubl'd that he can do it, dares not but Adverti ſe the Reader, That ſome Pages, partly a little after the beginning, and partly about the middle of the following Treatiſe, having been loſt through the negligence or miſtake of him to whoſe Care the ſheets whereon it was written were committed; he fears he has not been able, otherwiſe than very lamely and imperfectly, to repair that loſs out of his Memory.

THE

THE HISTORY OF *Fluidity* and *Firmneſs.*

The Firſt Part.

Of FLUIDITY.

SECT. I.

WHether Philoſophers might not have done better in making Fluidity and Firmneſs rather States then Qualities of Bodies, we will not now examine. But under which ſoever of the two Notions we look upon them, 'tis manifeſt enough, that they are to be reckon'd amongſt the moſt general Affections of the Conventions or Aſſociations of ſeveral particles of matter into Bodies of any certain denomination, there being ſcarce any diſtinct portion of matter in the World that is not either Fluid, or elſe

Stable or Conſiſtent. And therefore, I preſume, it may be well worth while to conſider what may be the general cauſes of theſe two States, Qualities, or Affections of Matter; and to Try whether by aſſociating Chymical Experiments to Philoſophical Notions, there may not be given at leaſt a more Intelligible and more Practical of both theſe Subjects, than has been hitherto afforded us by the Doctrine of the Schools, which is wont to appear very unſatisfactory to diſcerning Men; many of whom look upon what is wont to be taught by the Peripateticks, concerning Fluidity and Firmneſs, as well as other Qualities, to be partly too general to teach us much, and partly too obſcure to be underſtood. And that which at preſent invites us to this Enquiry is, chiefly, that ſome Circumſtances of our Authors Experiment, touching Salt-Petre, may afford us ſome uſeful aſſiſtance in our deſigned ſearch. For though the chief Phænomena and Circumſtances of the Experiment may be thought principally to reſpect Fluidity; yet ſince that and Firmneſs are contrary Qualities, and ſince it is truly, as well as commonly, ſaid, That contraries ſurvey'd together ſerve to illuſtrate each other, it may reaſonably be hop'd, That the Light which the circumſtances juſt now related to may give to the Nature of Fluidity, may facilitate the knowledge of that of Compactneſs: nevertheleſs, we ſhall often be oblig'd to treat of theſe two qualities together, becauſe the Experiments we are to produce do many of them relate to both.

SECT. II.

A Body then ſeems to be Fluid, chiefly upon this account, That it conſiſts of Corpuſcles that touching one another in ſome parts only of their Surfaces (and ſo being incontiguous in the reſt) and ſeparately Agitated to and fro, can by reaſon of the numerous pores or ſpaces neceſſarily left betwixt their incontiguous parts, eaſily glide along each others ſuperficies, and by reaſon of their motion diffuſe

fuſe themſelves, till they meet with ſome hard or reſiſting Body; to whoſe internal ſurface, by virtue of that Motion, their Smallneſs, and either their Gravity, or ſomething Analogous or Equivalent to it, they exquiſitely, as to ſenſe, accommodate themſelves.

SECT. III.

What notion *Epicurus*, and the Antient Atomiſts his followers, had of fluid Bodies, may be learn'd from theſe Verſes of his Paraphraſt *Lucretius:*

Illa autem debent ex lævibus atque rotundis
Eſſe magis, fluido quæ corpore liquida conſtant.
Nec retinentur enim inter ſe glomeramina quæque,
Et procurſus item in proclive volubilis extat.

And indeed, it is probable enough that in divers Liquors the little ſurfaces of the component particles are ſmooth and ſlippery, and that their being ſo does much facilitate the gliding of the Corpuſcles among themſelves; and conſequently, the Fluidity of the Body they compoſe. Nor is it to be deny'd, that the Spherical figure of ſuch Corpuſcles may alſo conduce to their eaſie rouling upon one another: but there are divers other figures which may make the little Bodies indow'd with them voluble enough to conſtitute a fluid ſubſtance. And the other qualities to be met with in divers liquid ſubſtances, and even in water it ſelf, and Oyl, ſeem to argue their parts to be otherwiſe ſhap'd, and thoſe fluid Bodies which are not Liquors, as Air and Fire, ſeem to be compos'd of particles not all or moſt of them round, but of very various, and ſometimes of very irregular figures, and yet that ſuch Bodies deſerve to be call'd fluid ones, will be manifeſt anon: And that they make a much more conſiderable part of the Univerſe than thoſe that are wont to be call'd Liquors, may be argu'd from hence, that except the Earth, the Planets, and perhaps

haps too the fixt Stars, the reſt of the World, as vaſt as it is, ſeems to conſiſt chiefly, if not only, of an Ætherial, thin, and fluid ſubſtance, as may appear (to omit other arguments) by what latter Aſtronomers have obſerv'd concerning the free and unreſiſted motion of ſuch Comets as have by a Trajection through the Æther, for a long time wander'd through the Celeſtial or Interſtellar part of the Univerſe.

SECT. IV.

And here let us obſerve, that 'tis not neceſſary to the Fluidity of a Body, nay, nor to its appearing fluid to the Eye it ſelf, That the Corpuſcles it conſiſts of be crowded as cloſe together as they are wont to be in water, and other bodies that are commonly lookt upon as the only Liquors. For though a parcel of matter no bigger than a grain of Corn, being rarify'd into ſmoak, will poſſeſs an incomparably greater ſpace than it did before; and though, if a Body be further rarify'd into flame, its expanſion will be yet much greater: yet both ſmoak and flame may be ſo order'd, as to appear like Liquors. We have practis'd divers ways, to make the fumes of Bodies acquire a viſibly-level ſuperficies like water; but the eaſieſt, though not perhaps the beſt, is this, (part of which I remember I have ſeen perform'd as a kind of trick by a very ingenious Perſon.) The mouth being fill'd with the ſmoak of Roſemary (that happening to be at hand when I made the Experiment) if this ſmoak be plentifully blown into a glaſs Pipe of an indifferent ſize, and open at both ends; and if when 'tis well fill'd with ſmoak, the lower end be preſently ſtopt, and the glaſs be kept ſtill a while in an erected poſture, the fumes will ſettle by degrees to a level ſuperficies like water: ſo that, though you gently incline the Pipe any way, the upper ſurface of the ſmoak will nevertheleſſe quickly grow parallel to the Horizon. And if the glaſſe be further (but ſlowly) made to ſtoop, the ſmoak will ſeem to run down in a Body like water, whilſt it continues

in

in the Pipe, though when it is come to the lower end of it, inſtead of dropping down like water, it will commonly rather flye upwards and diſperſe it ſelf into the Aire. And as for flame, I fore-ſee I ſhall ere long have occaſion to mention an Experiment, whereby I have ſometimes endeavour'd to ſhew, that ev'n two contiguous flames, as expanded Bodies as they are, and as open as their Texture is, may like viſible Fluids of a differing kind retain diſtinct ſurfaces.

SECT. V.

But inſtead of Examining any further, how many Bodies are or may be made viſibly to appear fluid ones; let us now reſume the Conſideration of what it is that make Bodies fluid; eſpecially, ſince having intimated ſome of the Reaſons, why we are unwilling to Confine our ſelves to the Epicurean notion, we hope it will the leſſe be diſlik'd, that we thought fit to make ſuch a deſcription of a fluid ſubſtance, as may intimate, that we conceive the conditions of it to be Chiefly theſe Three.

The firſt is the Littleneſſe of the Bodies that compoſe it: For in big parcels of matter, beſides the greater inequalities or roughneſſes that are uſual upon their ſurfaces, and may hinder the eaſie ſliding of thoſe Bodies along one another; and beſides that diverſe other Affections of a fluid Body cannot well belong to an aggregate of groſſe Lumps of matter; beſides theſe things, I ſay, the bulk it ſelf is apt to make them ſo heavy, that they cannot be agitated by the power of thoſe cauſes (whatever they be) that make the minute parts of fluid Bodies move ſo freely up and down among themſelves: whereas it would ſcarce be believ'd, how much the ſmallneſſe of parts may facilitate their being eaſily put into motion, and kept in it, if we were not able to confirme it by Chymical Experiments. But we ſee that Lead, Quick-ſilver, and ev'n Gold it ſelf, though whilſt they are of a ſenſible bulk, they will readily

ſink

ſink to the bottom of *Aqua Regis*, or any other ſuch Liquor; yet when the *Menſtruum* has corroded them, or fretted them aſunder into very minute parts, thoſe minute Corpuſcles grow then ſo much more capable of agitation than before, that quitting the bottom of the Liquor, they are carri'd freely every way, and to the top, with the aſſociated parts of the Liquor, without falling back again to the bottom. Nay, we ſee that ponderous and mineral Bodies divided into corpuſcles ſmall enough, may be made ſo light and voluble, as to become Ingredients ev'n of diſtill'd Liquors; as we may learn by what ſome Chymiſts call the Butter, others (ſimply) the Oyle, and others the *Oleum Glaciale* of Antimony, which though it be after Rectification a very limpid Liquor, yet does in great part conſiſt of the very Body of the Antimony, as may appear (not to mention its weight) by this, that 'tis moſt eaſie to precipitate out of it with fair water ſtore of a ponderous white calx, reducible by Art to an Antimonial glaſſe. Nay, we make a *Menſtruum*, with which we can eaſily at the firſt or ſecond Diſtillation bring over Gold enough to make the diſtill'd Liquor appear and continue ennobled with a Golden Colour.

And to ſhow yet more particularly, that great Bodies are too unwieldy to conſtitute fluid ones; We may further obſerve, how as well Nature as Art, when either of them makes Bodies of conſiderable bulk fluid, is wont in order thereunto, to make a Comminution of them, as we may obſerve in divers Examples.

SECT. VI.

Thus we ſee that in the ſtomacks of Dogs, Nature, to reduce Bones into thoſe fluid Bodies, Chyle and Blood, does by ſome powerful and appropriated juice, (whether belonging to the Stomack it ſelf, or thrown out of the Arteries in the paſſage of the circulating Blood) diſſolve them into parts ſo minute, that the acuteſt Eye would not tempt

tempt a man to ſuſpect, that ſuch a Liquor had ever been a Bone. And that it may not be objected that this diſſolution is chiefly performed, or at leaſt muſt always be aſſiſted by the Liquor which Animals take into their Stomachs by drinking; I ſhall repreſent not only that we find by experience how little, common water the only uſual drink of Dogs, Wolves, *&c.* is able to diſſolve bones though they be very long not macerated but boil'd in it; but that (if we may believe Natural Hiſtorians and credible Travellers) there are ſome ſorts of Animals, as particularly Camels, that may be brought not to drink once in many days, ev'n when they travel in hot Climates. And to make you think this the leſs improbable, I ſhall adde, that I am familiarly acquainted with an Ingenious Gentleman, who, as himſelf and an ancient Virtuoſo, in whoſe houſe he lives, have inform'd me, does uſually drink but once in ſeveral days, and then no exceſſive draught neither. And when I aſkt him how long he had actually abſtain'd not barely from drink, but from thirſting after it? He anſwer'd, that he had once (ſome few years before) continued about nine days without either taking or needing any drink; and he doubted not but that he might have continued much longer in that ſtate, if by diſtempering himſelf one night with long and hard ſtudy he had not had ſome light inclination to take a ſmall draught, which ſerv'd him for about four days longer. And when I aſkt him whether in that hot Summers day that preceded the evening wherein he happen'd to tell me this, he had not drunk at all? he anſwer'd Negatively. And it adds to the ſtrangeneſs of this Peculiarity, that this Gentleman is in the flower of his Youth, being but about twenty two years of Age, and of a Sanguine and Florid Complexion. And (to annex that alſo upon the By) I learned by enquiry from him, that he ſweats freely enough, as I remember I ſaw him do, that his Diet is the ſame with other mens, without re-

ſtraining him from the free uſe of Salt Meats, and that his Urine is in Quantity much like that of ordinary Men of his Age and temperament. But to return to what I was ſaying more generally of the Stomachical *Menſtruum* of Animals; I ſhall adde on this occaſion, that to make ſome kind of Imitation of it, I prepar'd, and do elſewhere mention and teach a certain Liquor, that I uſe, whereby I have in a ſhort time, and without fire, diſſociated the parts of roſted or boil'd fleſh, bread, fruit, *&c.* and pull'd them aſunder into very minute Bodies, whereby I have reduc'd ſometimes one, ſometimes another of them, together with the Menſtruum, (which needs not much exceed them in Bulk) to the conſiſtence of a fluid Body.

We ſee likewiſe, that Fuſion makes metalls fluid, and in Fuſion there is manifeſtly a comminution of the melted Body, the Heat alone of Gold, Silver, or Iron, though encreas'd even to Ignition, being not able to make thoſe metalls become fluid, whilſt they continue in maſſes of any ſenſible bulk. To which I ſhall adde anon, that even melted Metalls may have their Fluidity encreas'd by a yet further Comminution of their parts.

SECT. VII.

And to reſume here the Conſideration of that very difficult Queſtion, which we have elſewhere mention'd, it ſeems well worth Enquiry, whence it happens that in the diſtillation of common Salt and other ſaline Bodies, which not only are not fluid, but are hard ev'n to brittleneſs, there will yet be obtain'd a perfect and permanent Liquor, and from ſome of them a very conſiderable proportion of it. In anſwer to this Queſtion it may indeed be ſaid, That in diverſe dry Bodies, ſuch as Harts-horn, Wood and Bones, committed to diſtillation, the fire does no more than ſeparate the aqueous or other liquid parts from the others, wherewith they were blended in the Concrete, and bring them together into the Receiver, where they convene

convene into a Liquor. But beſides that this it ſelf is perhaps more eaſily ſaid than prov'd, it does not reach the propounded Difficulty: For with what probability can it be affirm'd of Bodies that have been already calcin'd or melted? ſuch as are the red Calx of Vitriol, and flux'd Sea Salt, &c. which yet afford Liquors, though their aqueous and other looſer parts have been already driven away by a ſtrong fire before their being expos'd to diſtillation. I have ſometimes then conſider'd, whether it may not ſeem leſs improbable to conjecture, that the vehement agitation produc'd in ſuch Bodies by the violence of Heat, does both divide them into minute Corpuſcles, and drive over ſwarms of them into the cold Receiver, where looſing their former vehemence of agitation, they are reduc'd into a Liquor, chiefly, (for I would not exclude concurrent cauſes) by reaſon that the fire happen'd to rend the Concrete into parts, by their extreme littleneſs, or their ſhape, or both, ſo eaſie to be tumbled up and down, that the wonted agitation of the Air, propagated by the interpos'd Bodies or Medium; or elſe that the ſame cauſe, whatever it be, that gives the Air its wonted agitation, is able to give ſuch minute Corpuſcles enough of it to keep them fluid.

SECT. VIII.

That there is conſtantly in the Air a various motion of the ſmall parts, will be anon declared. That alſo ſome Bodies will be kept fluid by a much leſs meaſure of agitation than is requiſite to others, ſeems probable from hence, That Wine will continue a Liquor in ſuch a languid warmth of the Air, as will not keep the parts of water moving, but permit them to reſt in the form of Ice. And in cold Countries, where Wine it ſelf would congeal, (as I have by Art made it do here in *England*) 'tis obſerv'd, that though the more aqueous parts will by the loſs of their motion be turn'd into Ice, yet the more ſubtil and

ſpirituous parts remain unfrozen; and ſo do diverſe other Liquors, (eſpecially Chymical) of very ſubtil and voluble parts. And the Corpuſcles that chiefly compoſe that Body which is properly call'd the Air, though it appears by weather-glaſſes that Cold may very much contribute to condenſe it, (that is, to occaſion the approach of its parts to one another, or, reduce them to a cloſer order) have not been obſerv'd to be frozen by any degree of cold whatſoever; which ſeems to proceed from hence, that by reaſon of their extream littleneſs, (not excluding their figure) there cannot be ſo little of agitation about the Earth, as not to be ſufficient to continue a various motion in ſuch very minute Bodies, and conſequently to keep them fluid.

Now, That likewiſe it is poſſible that a ſaline Spirit ſhould conſiſt at leaſt in great part of very minute grains of Salt, we elſewhere declare, where 'tis taught, that a Sal-Armoniack may be made by Spirit of Urine and Spirit of Salt, as the common Sal-Armoniack is made with crude Salt: and there a way is alſo ſhewn, how theſe two Salts, (the Urinous and the other) as ſtrictly as they are united in the compound, may be readily divorc'd. And agreeably to this I obſerve, that as (according to what I elſewhere note) a common *Aqua fortis* may be enabl'd to diſſolve Gold, (on which of it ſelf it will not faſten) by the addition of Spirit of Salt; ſo I find that common crude Salt barely diſſolv'd in it, will give it the like power of working upon Gold. Nay, I have try'd that crude Nitre diſſolv'd in good Spirit of Salt, may make it ſerve for an *Aqua Regis*. And I remember on this occaſion, that having enquir'd of the moſt noted Perſon in *Holland* for the diſtilling of corroſive waters, what was the greateſt proportion of diſtill'd Liquors that ever he was able to obtain from Sea Salt; he (though a man not given ſo much as to boaſting) affirmed to me, that by uſing inſtead of the ordi-

nary

nary *Caput mortuum*, as Brick-duſt, Sand, *&c.* (that Chymiſts are wont to mingle with Salt before they diſtil it) a certain whitiſh clay, he had ſometimes brought over almoſt the whole body of Salt into a Liquor; inſomuch that from a pound of Salt he could draw, and that without any extraordinary trouble or degree of fire, fourteen Ounces of Liquor. And when, becauſe I ſuſpected that much of this might be water forc'd from the clay mingled with the Salt, I enquired whether he had ever dephlegmed this Liquor; He anſwered me, that he had purpoſely done it; and ſometimes found no leſs than about twelve ounces of it to be ſtrong rectify'd Spirit: which brought into my mind that almoſt incredible paſſage of *Beguinus*, who ſomewhere teaching the Diſtillation of another Salt, addes to the end of his Directions, That if you have wrought well, you ſhall get from a pound of the matter a pound of Spirit. But becauſe from all theſe Liquors diſtill'd from ſuch kind of Salts, 'tis poſſible either by Rectification or ſome more Philoſophical way to obtain a portion of phlegm or water, I leave it to further Enquiry, whether or no the Fluidity of theſe diſtill'd Liquors may not in diverſe caſes be in part further'd by the mixture of ſome particles of an aqueous nature, (ſuch being fit to make Diſſolvers and vehicles for Salts) which may not abſurdly be ſuſpected to have been produc'd by the action of the fire upon the Concrete committed to diſtillation; if we allow with that famous Chymiſt *Helmont*, That by the Alchaheſt all groſs Bodies may be totally, and that without it, ev'n Oyle and Salt may in great part, (and that without Additaments) be reduc'd into inſipid water.

SECT. IX.

We ſhall anon, (when we come to treat of Firmneſs) mention our having made a certain ſubſtance ſo diſpos'd to Fluidity, that it may be made to change the ſtable conſiſtence for a liquid one, by ſo ſmall an Agitation, as only the

the Surpluſage of that which the ambient Air is wont to have about the middle even of a Winters day, above what it hath in the firſt or latter part of it. Nay, we have made ev'n a Metalline Salt or Vitriol, capable of this proclivity to liquefaction, of which we have unqueſtionable witneſſes. And therefore, it need not appear incredible, that other heaps or aggregates of Corpuſcles much lighter than theſe, though heavier than thoſe of the Air, may have all their parts ſo minute and fitted for motion, that the wonted agitation of the Air may not only about noon, but at all other times of the day keep them in motion, and thereby in the ſtate of Fluidity.

SECT. X.

And here I muſt adde, that 'twas not altogether without cauſe that I lately took notice of the ſhapes as well as the ſizes of Bodies, in reference to their fitneſs to conſtitute fluid ones. For though I be not ſure but that in thoſe Bodies, as Sal-Armoniack, Antimony, *&c.* which are by the fire ſublim'd into flowers rather than diſtill'd into Liquors, the magnitude of the component Corpuſcles may not be a hinderance to the Fluidity of the Body they conſtitute: yet this ſeems as probably referable to their figure, unapt for the requiſite motion, as to their bulk. And I have ſometimes made to this purpoſe this Experiment. That by ſlowly diſtilling Oyl-Olive *per ſe* in a glaſs Retort, (plac'd in Sand) I found, as I expected, that about the third part of the Oyle, which was driven over into the Receiver, did there coagulate into a whitiſh Body almoſt like Butter. So that although it ſeem'd manifeſt by the ſtrong ſmell and very piercing taſte of this white ſubſtance, that the Oyle which afforded it had its particles, as it were, torn in pieces; and though diſtillation be wont to obtain Liquors ev'n from conſiſtent Bodies; yet in our Experiment of a concrete that is naturally fluid, the diſtill'd Liquor it ſelf proves not to be ſo: of which no cauſe

cauſe ſeems more obvious, than that the newly-acquired ſhape of the diſſipated parts of the Oyly Corpuſcles makes them unfit for motion; either Abſolutely ſpeaking, or at leaſt in Reſpect of one another, by making them leſs pliant than formerly, or giving them a figure more eaſie to be entangl'd with the neighbouring Corpuſcles, or elſe by making their ſurfaces leſs ſmooth and ſlippery than before.

SECT. XI.

But to return thither whence we have digreſs'd, and mention ſome more familiar Examples of the Conduciveneſs of the ſmallneſs of a Bodies disjoyned parts to its Fluidity, we may take notice, that of Bodies that conſiſt of incoherent parts, and are made up, as it were by Aggregation, thoſe *de cæteris partibus*, in their being pour'd out moſt reſemble Liquors, that are the ſmalleſt; as would appear upon the emptying of ſeveral Sacks, the one of Apples, the other of Walnuts, the third of Filberts, the fourth of Corn, the fifth of Sand, and the ſixth of Flowre.

Confectioners alſo, Cooks, and others that make much uſe of whites of Eggs, will eaſily reduce thoſe clammy and viſcous Bodies into a thin and fluid ſubſtance, to which for its affinity with water many give the ſame name: and yet this difference of Fluidity being effected only by long and skilfully beating the maſs with a whisk, or even with a ſpoon, ſeems to be produc'd but by pulling aſunder the parts, (which perhaps before were long and ſomewhat twin'd) and breaking them into ſhorter or leſſer, and conſequently more voluble ones. And I remember, I have ſeen a good quantity of that jelly that is ſometimes found on the ground, and by the Vulgar call'd a Star-ſhoot, as if it remain'd upon the extinction of a falling Star, which being brought to an eminent Phyſician of my acquaintance, he lightly digeſted it in a well-ſtopt glaſs for a long time, and by that alone reſolv'd it into a permanent Liquor, which he extols as a ſpecifick to be outwardly apply'd againſt Wens.

SECT.

SECT. XII.

And here we will ſubjoin an Obſervation afforded us by the Art of Caſting, which has ſometimes yielded us a not unpleaſant Diverſion. 'Tis obſerv'd then by Gold-ſmiths well vers'd in that Art, (and has been recommended to me by an Artificer eminently skilful in it) as one of the chief Remarques belonging to it; that when any ſuch curious work of Silver is to be caſt, as requires that the impreſſion of hairs or very ſlender Lines be taken off by the Metal, it is not enough that the Silver be barely melted, but it muſt be kept a conſiderable while in a ſtrong fuſion: For if it be too ſoon pour'd out, the figure it will make will be but blunt; whereas if it be kept a competent time in Fuſion, the matter becoming thereby more Liquid as well as hotter, will be thin enough to run into the ſmalleſt cavities of the Mould, and ſo receive a figuration ev'n from the delicateſt of them. Whence it may probably be deduc'd, that ſome Bodies already fluid may by a further comminution of their parts be made yet more fluid. The like increaſe of Fluidity may be obſerv'd in ſome other fluid Bodies, eſpecially unctuous ones, as Turpentine, Oyle, *&c.* when heat begins to break as well as agitate their parts.

I may elſewhere have occaſion to mention, how by the operation of the fire the Cryſtalline Salt of Urine may be reduc'd without Additaments to a ſtrong and ponderous Liquor: though in this, as perhaps alſo in ſome of the former Inſtances, 'tis not unlikely that (as we may hereafter more particularly declare) there may concurre to the produc'd change of conſiſtence ſome alteration in the figure of the Corpuſcles whereof the firm Body conſiſted.

And if that be true which *Helmont* in ſeveral places affirms of his prodigious Liquor, *Alkaheſt*, it is poſſible to turn Plants, Animals, Stones, Minerals, Metals, or whatever kind you pleaſe of conſiſtent Body here below, into a Liquor

Liquor equiponderant to the reſolv'd Concrete: which (if granted) ſeems to argue, That the moſt ſolid Body by being divided into parts ſmall enough to be put into motion by the cauſes that keep thoſe of water and other Liquors in agitation, may become fluid Bodies. And this Intimation I ſhall adde for the ſake of Philoſophers, that barely by long Digeſtions, (and much more if they be help'd by ſeaſonably-repeated Diſtillations) in exactly ſtopt Veſſels, and a due degree of heat, there may be made in the parts of many Bodies, both Vegetable and Animal, ſo great a change from the ſtate of conſiſtence to that of Fluidity, as thoſe that contenting themſelves with ordinary courſes of Chymiſtry, have not had a peculiar curioſity for tryals of this nature, will not be forward to expect.

SECT. XIII.

The Second of the above-mention'd three Conditions is, That there be ſtore of vacant ſpaces intercepted betwixt the component particles of the fluid Body, or at leaſt about thoſe of them that are ſuperficial: for without this there will not be room for each of the Corpuſcles to continue its agitation upon the ſurfaces of the neighbouring ones; and there would be no Ceſſion of any, becauſe there would be no place unpoſſeſt for the impell'd Corpuſcle to be received in. But when I ſpeak of vacant ſpaces, ordinarily, (if not always) requiſite to be intercepted betwixt the particles of fluid Bodies; I intend not to determine whether or no ſuch ſpaces ſhould or may be vacuities properly ſo call'd; it being commonly ſufficient to this ſecond Condition of a fluid Body, that in the little ſpaces intercepted between thoſe that either are, or at leaſt are conſider'd as ſolid parts, there be none but ſuch as will eaſily yield to them, and cannot conſiderably reſiſt the freedom of their motions.

Which being premis'd to keep this Condition from being miſtaken, we may in confirmation of it take notice

SECT. XII.

And here we will ſubjoin an Obſervation afforded us by the Art of Caſting, which has ſometimes yielded us a not unpleaſant Diverſion. 'Tis obſerv'd then by Gold-ſmiths well vers'd in that Art, (and has been recommended to me by an Artificer eminently skilful in it) as one of the chief Remarques belonging to it; that when any ſuch curious work of Silver is to be caſt, as requires that the impreſſion of hairs or very ſlender Lines be taken off by the Metal, it is not enough that the Silver be barely melted, but it muſt be kept a conſiderable while in a ſtrong fuſion: For if it be too ſoon pour'd out, the figure it will make will be but blunt; whereas if it be kept a competent time in Fuſion, the matter becoming thereby more Liquid as well as hotter, will be thin enough to run into the ſmalleſt cavities of the Mould, and ſo receive a figuration ev'n from the delicateſt of them. Whence it may probably be deduc'd, that ſome Bodies already fluid may by a further comminution of their parts be made yet more fluid. The like increaſe of Fluidity may be obſerv'd in ſome other fluid Bodies, eſpecially unctuous ones, as Turpentine, Oyle, *&c.* when heat begins to break as well as agitate their parts.

I may elſewhere have occaſion to mention, how by the operation of the fire the Cryſtalline Salt of Urine may be reduc'd without Additaments to a ſtrong and ponderous Liquor: though in this, as perhaps alſo in ſome of the former Inſtances, 'tis not unlikely that (as we may hereafter more particularly declare) there may concurre to the preduc'd change of conſiſtence ſome alteration in the figure of the Corpuſcles whereof the firm Body conſiſted.

And if that be true which *Helmont* in ſeveral places affirms of his prodigious Liquor, *Alkaheſt*, it is poſſible to turn Plants, Animals, Stones, Minerals, Metals, or whatever kind you pleaſe of conſiſtent Body here below, into a Liquor

Liquor equiponderant to the reſolv'd Concrete: which (if granted) ſeems to argue, That the moſt ſolid Body by being divided into parts ſmall enough to be put into motion by the cauſes that keep thoſe of water and other Liquors in agitation, may become fluid Bodies. And this Intimation I ſhall adde for the ſake of Philoſophers, that barely by long Digeſtions, (and much more if they be help'd by ſeaſonably-repeated Diſtillations) in exactly ſtopt Veſſels, and a due degree of heat, there may be made in the parts of many Bodies, both Vegetable and Animal, ſo great a change from the ſtate of conſiſtence to that of Fluidity, as thoſe that contenting themſelves with ordinary courſes of Chymiſtry, have not had a peculiar curioſity for tryals of this nature, will not be forward to expect.

SECT. XIII.

The Second of the above-mention'd three Conditions is, That there be ſtore of vacant ſpaces intercepted betwixt the component particles of the fluid Body, or at leaſt about thoſe of them that are ſuperficial: for without this there will not be room for each of the Corpuſcles to continue its agitation upon the ſurfaces of the neighbouring ones; and there would be no Ceſſion of any, becauſe there would be no place unpoſſeſt for the impell'd Corpuſcle to be received in. But when I ſpeak of vacant ſpaces, ordinarily, (if not always) requiſite to be intercepted betwixt the particles of fluid Bodies; I intend not to determine whether or no ſuch ſpaces ſhould or may be vacuities properly ſo call'd; it being commonly ſufficient to this ſecond Condition of a fluid Body, that in the little ſpaces intercepted between thoſe that either are, or at leaſt are conſider'd as ſolid parts, there be none but ſuch as will eaſily yield to them, and cannot conſiderably reſiſt the freedom of their motions.

Which being premis'd to keep this Condition from being miſtaken, we may in confirmation of it take notice

how Snow, which at its first falling is of a loose and open texture, does easily yield to the impressions of the hand: But when by being strongly compress'd and form'd into Balls, the little Icy bodies it consists of are brought into a closer order, and many of them thrust into the little spaces formerly possest only by the yielding Air, they become unable to give way to the motions of our hand as before, but compose a hard and resisting Body. We see also, that when water is strongly forced into and kept compressed in a Bladder, so that its exteriour particles have not about them as before the yielding Air to give way to them, when they should according to their wont swell about the sides of the Bodies that endeavour to press it inwards, it emulates a hard body, and resists such motions as otherwise it would readily yield to; unless a more easie Cession be occasion'd by the Retching of the moisten'd Bladder it self.

And I chuse to instance in a Bladder distended with water, rather than in one full of Air, because, though this latter will also emulate a hard Body, yet in this case the tention of the Bladder would perhaps be ascrib'd to a kind of Spring, which diverse Experiments have taught us to belong to the Air: whence it might be said, that since the enclos'd Air will suffer it self to be thrust inward a good way, though it will quickly when permitted flye out again; the hardness of a well-blown Bladder proceeds not from want of the rooms requisite to the Cession of the aerial Corpuscles, but to the motion of Restitution natural to them, when like an innumerable company of little Bows or Springs, being bent by the force that compresses the sides of the Bladder, they do as soon as it is taken off stretch themselves out again (some one way, some another) as far as is permitted them by the imprisoning bladder, which they thus every way keep strongly distended.

But

But this having of vacant ſpaces or ſome yielding matter about the Corpuſcles of a fluid Body, ſeems requiſite to its being ſo, but as what in a School-term one may call a *Removens prohibens*, I mean, only as it obviates that impediment to their motion, which exquiſite fulneſs may be conceiv'd to give to the various glidings amongſt themſelves of the parts of a Body ſuppos'd to be perfectly of the ſame hardneſs or ſoftneſs, or, if you pleaſe, altogether equally diſpos'd or indiſpos'd to yield to one another. And although in ſuch Bodies, as Water, Wine, Oyle, Quick-ſilver, and the like, that are generally agreed upon to be fluid Liquors, it will I preſume be granted, that this ſecond Condition we have been ſpeaking of may take place; yet I will not ſay that 'twere altogether abſurd to queſtion, whether there may not be a portion of matter conſiſting of parts ſo minute, and ſo agitated, and conſequently ſo eaſie to be either crumbl'd into yet ſmaller parts, or ſqueez'd into any figure as occaſion requires, that they may inceſſantly change places among themſelves, and thereby conſtitute a moſt fluid Body, without any vacuities, receptacles, or yielding matter about them, unleſs perhaps it be about the exteriour parts of thoſe of them that from time to time happen to be the ſuperficial Corpuſcles of this thinneſt Liquor. But though we have ſaid, that this may be queſtion'd without abſurdity, yet it will not ſo much concern us in this place to examine whether the affirmative may be rationally maintain'd, as to proceed to conſider what is farther requiſite to that ſtate of matter we are now treating of, eſpecially the Qualification yet unmention'd ſeeming to be the principal of all.

SECT. XIV.

For the Third and Chief Condition of a fluid Body is, that the particles it conſiſts of be Agitated Variouſly and Apart, whether by their own innate and inherent motion, or by ſome thinner ſubſtance that tumbles them about in

its paſſage through them. For this ſeems to be the main difference betwixt ſolid Ice and fluid Water, that in the one the parts (whether by any newly acquir'd texture, or for want of ſufficient heat to keep them in motion) being at reſt againſt one another, reſiſt thoſe endeavours of our fingers to diſplace them, to which in the other the parts being already in motion eaſily give way. For whereas in the Ice, every part actually at reſt muſt by the Law of Nature continue ſo, till it be put out of it by an external force capable to ſurmount its reſiſtance to a change of its preſent ſtate; in Water each Corpuſcle being actually (though but ſlowly) mov'd, we need not begin or produce a new motion in it, but only byaſs or direct that which it has already, which many familiar Inſtances manifeſt to be a much eaſier task. From this Agitation of the ſmall parts of Liquors it comes to paſs, that theſe little Bodies, to continue their motion, do almoſt inceſſantly change places, and glide ſometimes over, ſometimes under, and ſometimes by the ſides of one another. Hence alſo may be render'd a reaſon of the ſoftneſs of fluid Bodies, that is, their yielding to the touch; for the particles that compoſe them being ſmall, incoherent, and variouſly mov'd, it can be no difficult matter (as we lately intimated) to thruſt them out of thoſe places, which being already in motion they were diſpos'd to quit, eſpecially there being vacant rooms at hand, ready to admit them as ſoon as they are diſplac'd. And hence it likewiſe happens, that theſe little Bodies muſt be very eaſily moveable any way upon the motion of the maſs or Liquor which they compoſe; and that being very ſmall, and moving ſo many ways, they cannot but (according to *Ariſtotle*'s Definition of things fluid) be very unfit to bound themſelves, but very eaſie to be bounded by any other firm Body; for that hinders them from ſpreading any further: and yet to continue their various and diffuſive motion as much as they can,

(eſpecially

(especially their gravity, at least here about the Earth, equally depressing and thereby levelling as to sense their uppermost superficies) they must necessarily move to and fro, till their progress be stopt by the internal surface of the Vessel, which by terminating their Progress (or Motion toward the same part) does consequently necessitate the Liquor those little Bodies compose, to accommodate it self exactly (for ought the Eye is able to discern to the contrary) to its own figure.

SECT. XV.

This short and general Account of Fluidity may we hope be as well further explicated and illustrated, as confirmed, by the following Instances and Experiments, and therefore we shall forthwith proceed to Them.

And it will be fit to mention in the first place those that are afforded us by the Body our Author treats of, Salt-Petre, they having occasioned our writing about this Subject.

Salt-Petre then may be made fluid two several wayes, either by, or without a Liquor.

By the intervention of a Liquor it puts on the form of a fluid Body, when being dissolv'd in water or aqueous juices, it is not by the Eye distinguishable from the solvent Body, and appears as fluid as it; which seems to proceed from hence, that the agitated particles of the water piercing into the joints or commissures of the Corpuscles of the Salt, do disjoyn them, and thereby divide the Nitre into parts so small, that it is easie for those of the water, wherewith they are associated, not only to support them, but move them to and fro: whence it comes to pass, that these Particles being so small, and swimming some one way, some another in the yielding body of water, make no such resistance against the motion either of a mans hand or other external Body that strives to displace them, as they did in their saline form.

But

But that with much leſs Liquor a Nitrous body may be rendred fluid, may appear to him that ſhall expoſe ſuch fix'd Nitre as our Author teaches to make, to the moiſt Air of a Cellar: For there it will run *per deliquium*, (as Chymiſts ſpeak) into a Liquor, which conſiſts of no more aqueous Particles than are neceſſary to keep the ſaline ones (which ſeem to be much ſmaller than thoſe of unanalyz'd Nitre) in the agitation requiſite to Fluidity.

SECT. XVI.

And hence we may proceed to conſider, what Fluidity Salt-Petre is capable of without the intercurrence of a Liquor: and this may be two-fold. For firſt, if it be beaten into an impalpable powder, this powder, when it is pour'd out, will emulate a Liquor, by reaſon that the ſmallneſs and incoherence of the parts do both make them eaſie to be put into motion, and make the pores they intercept ſo ſmall, that they ſeem not at a diſtance to interrupt the unity or continuity of the Maſs or Body. But this is but an imperfect Fluidity, both becauſe the little grains or Corpuſcles of Salt, though eaſily enough moveable, are not alwaies in actual motion; and becauſe they continue yet ſo big, that both they and the ſpaces intercepted betwixt them are, near at hand, perceivable by ſenſe. But if with a ſtrong fire you melt this powder'd Nitre, then each of the ſaline Corpuſcles being ſub-divided into I know not how many others, and theſe inſenſible parts being variouſly agitated by the ſame heat, (both which may appear by their oftentimes piercing the Crucible after fuſion, wherein they lay very quietly before it) the whole body will appear a perfect Liquor, and be thought ſuch by any Beholder that ſhall judge of it but by the Eye: and ſuch alſo is the Fluidity of melted metals, in which, when they are brought to fuſion in vaſt quantities, I have ſeen the ſurface wav'd like that of boyling water, and ſometimes parcels of Liquor thrown up a pretty way into the Air. And not

not only Fire and other actually and manifeſtly hot Bodies are able to make ſome hard ones fluid, but it ſeems alſo that ſome bodies may be brought to Fluidity by others which to the touch appear cold, if they be but fitted to change the texture of the hard body, and put its inflected parts into a convenient motion; as may be ſeen in the Chymical Experiment of turning the brittle body of Camphire into an Oyl for the time, by letting it lye upon *Aqua fortis*, which perhaps bends and complicates the formerly rigid particles, and puts them into ſuch a motion, that they do as well glide along as ſomewhat twine about each other. And I further try'd, (not having found it menti-on'd by the Chymiſts) that Camphire may by a dexterous application of heat be brought in cloſe glaſſes both to flow and to boyl almoſt like Oyl. 'Tis true, that theſe Li-quors taken from the fire quickly loſe that name, and grow ſolid again. But the duration of a thing is not always ne-ceſſary to denominate it ſuch; for the Leaf of a Tree, for inſtance, whilſt it flouriſhes, may be as truly green as an Emerald, though the leaf will after a while wither and turn yellow, which the ſtone will never do; and in cold Climates, where Lakes, *&c.* at other times navigable, are ſometimes frozen ſo hard, that Carts and ev'n great Ord-nance may ſafely be drawn over them, Ice and water are the one a ſtable, and the other a liquid Body, notwith-ſtanding that the ſame portion of matter which at one time is frozen into a hard and ſolid ſubſtance, was a little before a fluid Body, and (now and then in a very ſhort time) will be thaw'd into a Liquor again.

SECT. XVII.

I know not whether it be requiſite to take notice, that the Fluidity which Salt-Petre acquires upon fuſion by fire ſeems very much of kin to that which is acquir'd by ſolu-tion in water. But if fuſion be made rather by the Ingreſs and tranſcurſions of the atoms of fire themſelves, than by the

the bare propagation of that motion with which the agitated particles that compoſe fire beat upon the out-ſide of the veſſels that contain the matter to be melted; in ſuch caſe, I ſay, both thoſe kinds or manners of Fluidity newly aſcrib'd to Salt-Petre will appear to be caus'd by the pervaſion of a foreign body: Only in diſſolution the fluid body is a Viſible and Palpable Liquor, and conſequently more groſs, whereas in fuſion the fluid ſubſtance that permeates it is more thin and ſubtil, and divides it into much ſmaller parts, and ſo adds very little to its bulk.

SECT. XVIII.

But becauſe ſome ſcruple may poſſibly ariſe about this matter from hence, that the powder of Nitre, how fine ſoever, ſeems fluid but juſt whilſt it is pouring out, and ev'n then is but very imperfectly ſo; and that as for fuſion, that is wont to reduce the melted body to a new and permanent ſtate, as the formerly-mention'd powder of Salt-Petre, which before fuſion was but a heap of incoherent particles, is by it made a ſolid and conſiderably hard Body: to prevent, I ſay, or remove ſuch ſcruples, we will ſet down one Experiment that we long ſince met with, as to the main, in the ſhops of Stone-Cutters, which though unregarded by them, will excellently ſerve to make out what we mention it for. Take then good Alabaſter, or in defect of this, of that white ſtone which is well known to our Maſons by the name of Plaſter of *Paris*, beat it very ſmall, and put as many pounds as you pleaſe of the finely-ſearc'd powder into any flat-bottom'd (and firſt well heated) veſſel of Braſs or Iron (bigger or leſſer according to the quantity you intend to burn:) Encreaſe the fire by degrees till it grow to be ſtrong, and when the calorifick Atoms ſhall have in ſufficient numbers pervaded the heap of powder, or, if you pleaſe, when the igneous Corpuſcles have by their numerous and briſk ſtrokes upon the veſſel communicated by its means their agitation to the encloſed powder, and

and when by either of theſe ways, or both, the fire (which may alſo reſolve ſome of the more ſpirituous and exhalable parts (whereof Diſtillation has ſhewn me that Alabaſter is not deſtitute) into Vapours) ſhall have put the little bodies it conſiſts of into actual motion (which will be quickly done) you ſhall ſee it aſſume the form of a Liquor, and boil with numerous great and confus'd waves juſt like a ſeething-pot: and if, whilſt it continues in this ſtate, you ſtir it with a ſtick, it will not like a heap of ſand, or as it ſelf would do at another time, reſiſt the motion thereof, but yield thereto like a Liquor, and, like it, will ſeem to have ſomething of the Nature of a coherent body; for by ſtirring it any thing ſtrongly near one ſide of the Veſſel, you may make the waves beat very manifeſtly againſt the oppoſite part of it. And beſides all this, you may obſerve this further reſemblance betwixt this boiling matter and a Liquor, that there will flye up out of the Pot great ſtore of ſteams like ſmoak, but that they are white, which will ſometimes like ſmoak aſcend, for ought can be diſcern'd, to the very top of the Chimney, and leave its colour upon the places by which plenty of it hath paſt. Beſides, thoſe that make this Experiment often, as we have taken pleaſure to do, may have the opportunity to obſerve, that when the Veſſel has continued ſo long over the fire that the contained Alabaſter relapſes into the form of a heavy moveleſs powder, by keeping it a while longer in the heat, it will for once at leaſt reſume the form of a fluid body, and boil again as before, the ſpirituous ſteams whoſe avolation promoted the Ebullition, being not yet quite ſpent. And laſtly, if when it ſeems moſt a Liquor, you take up a little of it, and as nimbly as you can caſt it upon a ſheet of white Paper, it will not at all wet it, but immediately diſcover it ſelf to be a moveleſs incoherent powder, as it was before its being ſet over the fire; whereby it (I hope) appears, that a heap or aggregate of ſuch little bodies as are

neither Spherical nor otherwiſe regularly ſhap'd, nor ſmall enough to be below the diſcernment of the Eye, may, without either fuſion or being pour'd out, be made fluid barely by a ſufficiently ſtrong and various agitation (from what cauſe ſoever that proceed) of the particles that make it up, and loſe its fluidity immediately upon the ceaſing of it.

Thus have we ſeen how very much it conduces to the making of a Body fluid, that its ſmal parts be actually mov'd. But whence this motion proceeds, we ſhall not at preſent venture to determine. For though in the Examples newly mention'd, and ſome others, moſt men will be forward to aſcribe the motion produc'd in the parts of the fluid Bodies there mention'd, to the action of the fire whereunto they were expos'd; yet what it is that puts the parts of fluid Bodies in general into the motion requiſite to make them ſuch, is a Queſtion of which the true Reſolution indeed were very deſirable. But the full debate of it will not, I hope, be here expected from me, whilſt I am writing but Notes, ſince it would engage me to diſcuſs two or three of the difficulteſt as well as the importanteſt Controverſies belonging to Natural Philoſophy. For firſt, I ſhould be oblig'd to conſider whether Motion, or a propenſity to it, be an inherent Quality belonging to Atoms in general, and not loſable by them; or whether all Motion is communicated by impulſe from one Body to another. And ſince thoſe that of late have taught that all viſible Liquors, as Water, Oyl, Quick-ſilver, &c. owe their fluidity chiefly to the agitation of ſome thin and reſtleſs matter which inceſſantly permeates them, do deduce the neceſſity of ſuch an Ethereal ſubſtance principally from the impoſſibility that there can be any *Vacuum* properly ſo call'd in the Univerſe, wherein yet are very many ſpaces unpoſſeſt by either Air or groſſer Bodies than it: the Examination of this ſubtil matter would draw on the Conſideration of the nice

nice Controverſies that perplex Philoſophers concerning Emptineſs, which 'twere more difficult for us to examine in few words, than it is neceſſary for us to meddle with them in this place; ſince not writing of the firſt Principles of Phyſiology, but of Fluidity, which is but a ſecondary or derivative quality (if I may ſo call it) it ſeems ſufficient to give a notion of it, that we deduce it not from the unintelligible ſubſtantial form of the fluid Bodies, but from thoſe ſimple and general Affections of Matter, the Figure, Situation, and Motion of its ſmall parts.

SECT. XIX.

Wherefore declining to adde any thing in this place to what we have otherwhere diſcourſed concerning the Origine of Motion, and the poſſibility or impoſſibility of a *Vacuum*; we will proceed to take notice that there is one thing more which we may learn from Salt-Petre touching the Nature of Fluidity, and that is the diſtinction betwixt a fluid Body and a wetting Liquor, which are wont, becauſe they agree in many things, to be confounded, but inconſiderately enough: for though every wetting Liquor be fluid, yet every fluid Body does not wet. The Air, the Æther, and ev'n flame it ſelf may be properly call'd Fluid Bodies according to the notion of Fluidity hitherto made out, and yet will ſcarce by any man be call'd Moiſt Liquors; and Salt-Petre, whilſt in fuſion, is really a Liquor, and ſo is every melted Metal, and yet theſe wet not the bodies they touch, as do Water and other wetting Liquors, which are fluid bodies with this peculiar qualification, that they ſtick to and moiſten the dry bodies which they touch (or at leaſt abound with ſome parts, which being ſeparated from the reſt and reduced to a Liquor, will do ſo.) And according to this notion, methinks, it may be conceiv'd, that the humidity of a body is but a relative thing, and depends chiefly upon the congruity or incongruence of the component Particles of the Liquor in

reference to the pores of thoſe particular bodies that it touches: for, ſometimes the little eminencies and pores of the ſurface of the dry body on or againſt which the Liquor flows, are of ſuch magnitudes and figure, that the particles of the Liquor find admittance into thoſe pores, and are detain'd there (by which means they uſually ſoften it;) and ſometimes the pores and aſperities of the dry bodies ſurface are ſo incommenſurate in bigneſs & figure to the particles of the Liquor, that they glide over the ſurface without ſticking or adhering firmly to any part of it. This may be exemplifi'd in Quick-ſilver, which cannot be ſaid to be a humid body in reſpect of our handsor cloaths, or of almoſt all other bodies of the World, upon whoſe ſurfaces it will roul without leaving any of its particles lodg'd in their pores, or faſtn'd to their little eminencies, whence it is called by vulgar Chymiſts, the water that wets not the Hands: but in reference to divers Metals, eſpecially Gold and Tin, Quick-ſilver may be ſaid to be a humid Liquor, for it inſinuates it ſelf into their pores, and thereby mollifies their bodies, as other Liquors do thoſe that are moiſtned by them. And even water, that wets almoſt all other Animal and Vegetable, and many mineral bodies, beſides that it is commonly enough obſerv'd to ſtand in almoſt globular drops upon Cabbage-leaves, ſeems not a humid Liquor in relation to the feathers of Ducks, Swans, and other water-fowl, whom Nature having deſign'd to flye ſometime in the Air, and live ſometimes in the water, ſhe providently makes their feathers of ſuch a texture, that they do not, like the feathers of divers other birds, admit the water, which imbib'd would make them unfit for the uſe of flying. And 'tis obſervable, that upon the change of texture in a Liquor, it may be brought to ſtick to the ſurface of a body to which before it would not adhere; as may appear by this, that though Quick-ſilver alone will not ſtick to glaſs, yet if there be mixt with it a due propor-

tion

tion of Lead, Tin, and Tin-glaſs, though neither of them will adhere to glaſs, yet their liquid mixture (as we have often tryed and elſewhere * taught) readily will, even without the aſſiſtance of heat.

* *The Writing here referred to, is the Second Tome of the Uſefulneſs of Experiment. Philoſophy, which ſhould have appeared before theſe* Specimens.

SECT. XX.

If it be objected, that this various agitation of the inſenſible parts of water and reſembling bodies wherein we make the Nature of Fluidity chiefly to conſiſt, is but an imaginary thing, and but precariouſly aſſerted, ſince by our own Confeſſion they are ſo ſmall, that the particles themſelves, and more, the diverſity of their motions are imperceptible by ſenſe, which repreſents water, for Example, to us as one continu'd body, whoſe parts are at perfect reſt;

If this, I ſay, be urged againſt our Doctrine, we ſhall not deny the Objection to be plauſible, but muſt not acknowledge it to be unanſwerable.

For of the ſeeming continuity of Water and other Liquors this may be the Reaſon, That the particles whereof the Liquor conſiſts, being too ſmall to be viſible, and being not only voluble, but in actual motion, the pores or vacant ſpaces intercepted between them, muſt alſo be too little to be diſcern'd by the Eye, and conſequently the body muſt appear an uninterrupted or continu'd one: not to mention, that, were the parts of the Liquor leſs minute, their ſhifting of places would hardly be perceiv'd by the Eye, each diſplac'd Corpuſcle being immediately ſucceeded by another like it. 'Tis true, that a heap of grains of Nitre, though upon its effuſion out of the Veſſel it ſomewhat emulates a fluid body, does yet when it reſts in the Veſſel appear to be but an aggregate of many little incoherent bodies heap'd up together; becauſe the intervals or holes left between them are great enough to affect the ſenſe:

But if the ſame Salt be reduc'd into an Alchoole (as the Chymiſts ſpeak) or impalpable powder, the particles and intercepted ſpaces being then extreamly leſſen'd, the body they make up will much more reſemble an intire maſs, though it be look'd upon from a nearer diſtance; and ſo when this powder is by the fire further broken into parts incomparably ſmaller than thoſe of the powder, and which conſequently intercept ſuch extreamly little pores, that not only Salt-Petre, but ſome Metals, and ev'n Gold it ſelf, (from which it will not be ſuppos'd that any thing exhales to leſſen it) are *by ſome* affirm'd (for I have not my ſelf diligently enough obſerv'd it, and do yet doubt it) to take up rather leſs than more room melted than cold, why ſhould we not grant that theſe pores may be little enough, not any where to diſcontinue the body as to ſenſe?

SECT. XXI.

And that the incoherent parts of fluid bodies are alſo diverſly agitated, ſome this way, and ſome that way, though the ſenſe cannot diſcern it, may be prov'd by their ſenſible operations. * For without ſuch local motion, how could the particles of water pierce into the receſſes of Bodies, and occaſion thoſe putrefactive alterations that are wont to be imputed to ſuperfluous moiſture? And how comes it elſe to paſs, that aqueous Liquors ſo readily diffuſe themſelves into, and ſo exquiſitely mingle with one another? as we ſee when red and white Wine are in a trice confounded into Claret: and without this various agitation of the parts of water, how could it be that lumps of Sugar or Salt caſt into it, ſhould quickly be ſo perfectly diſſolv'd in it, that the lumps themſelves totally diſappear, and the diſſociated

[* *The Author now finding that ſomething concerning the various motion of the parts of fluid Bodies, which he has but touch'd upon, has been, though but briefly and without Experiments, yet excellently explain'd in a mathematical way by* Monſieur des Cartes *in the* 56th *and* 57th *Articles of the ſecond Part of his Principles, thinks fit to refer the inquiſitive Reader thither for fuller ſatisfaction about that Particular.*]

ſociated parts are carried about every way by thoſe of the water, even from the bottom to the very top? as is evident particularly in Sea-ſalt, which when the ſuperfluous Liquor is ſufficiently exhal'd, begins viſibly to coagulate, not at the bottom, but upon the ſurface of the water; and not only Salt, but even Gold it ſelf, though the heavieſt of bodies, may have its parts ſo ſcatter'd by the agitation of thoſe waters, as Experience has taught us, and as you may eaſily try by putting a little of the Solution of Gold made in *Aqua Regis* into 15 or 20 times as much fair water, which will all thereby be immediately enobled with a Golden Colour. That the little bodies whereof flame conſiſts are fiercely agitated, appears oftentimes even to the Eye, and will ſcarce be denied by him that conſiders the operations of it, and the vivid beams it darts round about it againſt the neighbouring bodies. And that the particles that compoſe our common air are alſo very diverſly agitated, we may be induc'd to believe by ſundry particulars. As firſt, by thoſe little moats that from a ſhady place we ſee ſwimming up and down in the Sun-beams, and by the tremulous motion which that of ſwarms of little bodies in the air ſeems to impart to diſtant objects look'd on after Sunriſe through a good Teleſcope, (and which by the bare Eye in hot weather may be often diſcover'd by certain very dilute ſhades, which ſeem to tremble upon the walls of high-roof'd Halls and Churches, and other ſpacious Buildings.) Next, (and more eaſily) by this, That if you take Salt of Tartar, firſt brought to fuſion, and place it in a Cellar, or ev'n in an ordinary Room, it will in a ſhort time (now and then in a few minutes) begin to relent and have its ſurface ſoftn'd by the imbib'd moiſture of the air, wherein if it be left long enough, it will totally be diſſolved into clear Liquor; which would not be, if the moiſt vapours that help to conſtitute the air did not move to and fro every way, and were not thereby brought to the

Salt,

But if the ſame Salt be reduc'd into an Alchoole (as the Chymiſts ſpeak) or impalpable powder, the particles and intercepted ſpaces being then extreamly leſſen'd, the body they make up will much more reſemble an intire maſs, though it be look'd upon from a nearer diſtance; and ſo when this powder is by the fire further broken into parts incomparably ſmaller than thoſe of the powder, and which conſequently intercept ſuch extreamly little pores, that not only Salt-Petre, but ſome Metals, and ev'n Gold it ſelf, (from which it will not be ſuppos'd that any thing exhales to leſſen it) are *by ſome* affirm'd (for I have not my ſelf diligently enough obſerv'd it, and do yet doubt it) to take up rather leſs than more room melted than cold, why ſhould we not grant that theſe pores may be little enough, not any where to diſcontinue the body as to ſenſe?

SECT. XXI.

And that the incoherent parts of fluid bodies are alſo diverſly agitated, ſome this way, and ſome that way, though the ſenſe cannot diſcern it, may be prov'd by their ſenſible operations. * For without ſuch local motion, how could the particles of water pierce into the receſſes of Bodies, and occaſion thoſe putrefactive alterations that are wont to be imputed to ſuperfluous moiſture? And how comes it elſe to paſs, that aqueous Liquors ſo readily diffuſe themſelves into, and ſo exquiſitely mingle with one another? as we ſee when red and white Wine are in a trice confounded into Claret: and without this various agitation of the parts of water, how could it be that lumps of Sugar or Salt caſt into it, ſhould quickly be ſo perfectly diſſolv'd in it, that the lumps themſelves totally diſappear, and the diſſociated

[* *The Author now finding that ſomething concerning the various motion of the parts of fluid Bodies, which he has but touch'd upon, has been, though but briefly and without Experiments, yet excellently explain'd in a mathematical way by* Monſieur des Cartes *in the* 56th *and* 57th *Articles of the ſecond Part of his Principles, thinks fit to refer the inquiſitive Reader thither for fuller ſatisfaction about that Particular.*]

ſociated parts are carried about every way by thoſe of the water, even from the bottom to the very top? as is evident particularly in Sea-ſalt, which when the ſuperfluous Liquor is ſufficiently exhal'd, begins viſibly to coagulate, not at the bottom, but upon the ſurface of the water; and not only Salt, but even Gold it ſelf, though the heavieſt of bodies, may have its parts ſo ſcatter'd by the agitation of thoſe waters, as Experience has taught us, and as you may eaſily try by putting a little of the Solution of Gold made in *Aqua Regis* into 15 or 20 times as much fair water, which will all thereby be immediately enobled with a Golden Colour. That the little bodies whereof flame conſiſts are fiercely agitated, appears oftentimes even to the Eye, and will ſcarce be denied by him that conſiders the operations of it, and the vivid beams it darts round about it againſt the neighbouring bodies. And that the particles that compoſe our common air are alſo very diverſly agitated, we may be induc'd to believe by ſundry particulars. As firſt, by thoſe little moats that from a ſhady place we ſee ſwimming up and down in the Sun-beams, and by the tremulous motion which that of ſwarms of little bodies in the air ſeems to impart to diſtant objects look'd on after Sunriſe through a good Teleſcope, (and which by the bare Eye in hot weather may be often diſcover'd by certain very dilute ſhades, which ſeem to tremble upon the walls of high-roof'd Halls and Churches, and other ſpacious Buildings.) Next, (and more eaſily) by this, That if you take Salt of Tartar, firſt brought to fuſion, and place it in a Cellar, or ev'n in an ordinary Room, it will in a ſhort time (now and then in a few minutes) begin to relent and have its ſurface ſoftn'd by the imbib'd moiſture of the air, wherein if it be left long enough, it will totally be diſſolved into clear Liquor; which would not be, if the moiſt vapours that help to conſtitute the air did not move to and fro every way, and were not thereby brought to the Salt,

Salt, and enabled to insinuate themselves into its pores, and by that means dissolve it, and reduce it with themselves into a Liquor.

And even in Summer, when the air is wont to be much dryer than at other Seasons of the year, one may quickly discover that there are in the air store of aqueous Corpuscles, mov'd some one way and some another, by the Experiment of putting into a Drinking-glass, for want of Ice and Snow, some Beer or Wine actually very cold: for thereby, after a while, the outside will appear all bedew'd with little drops of Liquor; which seems plainly to be no other than the aqueous steams that swimming up and down in great multitudes in the air, are by its agitation towards all parts carried, as every other way, so to the sides of the Glass, and being there condens'd by the coldness of that smooth Body, turn into visible and palpable water. And, if I much mis-remember not, it was one of the circumstances of the last Experiment of this kind we have had occasion to take notice of, That the drops that fastn'd themselves to the outside of the Glass, purposely left in part unfill'd, reach'd either not at all, or very little further than the surface of the Liquor within the Glass, whose coldness as it seems did not infrigidate those upper parts of the Glass, to whose level the Liquor it self did not reach. To which I could easily adde Arguments to prove, that the drops we have been speaking of proceeded not from the transudation of the Liquor within the Glass, if I thought it worth while to disprove so unlikely a Conjecture. But instead of that I shall only intimate, that from this Experiment useful hints may be taken both Theorical and Practical, and particularly that a Reason may perchance be given of a strange way of catching a Salt and Liquor out of air, barely by glass-vessels of a peculiar and skilful contrivance. Much of what we have lately said will, I presume, be the less wonder'd at, if we subjoyn what

Expe-

Experience has taught us, That 'tis not difficult by the help of a convenient Furnace and fit Veſſels to make that ponderous Metal, Lead, aſcend to a good height in the open air, in the form of a copious ſmoak: ſuch a ſmoak we diſcern'd after a while to be carried ſo many ways by the aerial Corpuſcles that it met with in actual motion, that it was ſoon diſpers'd ſo far as to diſappear: which perhaps will be thought ſome confirmation of what we formerly deliver'd, when we taught how much the being divided into very minute parts may conduce to the Fluidity even of ponderous Bodies.

SECT. XXII.

And though Quick-ſilver be, excepting Gold, the heavieſt known body in the world, yet when it is reduc'd into vapour, it ſeems to be carried to and fro like the other terreſtrial particles that ſwim up and down in our air: for I remember, that an expert Gilder not long ſince complain'd to me, that if when he evaporated Quick-ſilver, he forgot to take off his Rings from his hand, though they touch'd not the Quick-ſilver whilſt it was in a body, the roving fumes would oftentimes faſten upon the Gold in ſuch plenty as would put him to much trouble to get them off from his Rings; one of which he ſhew'd me that he had lately thus whitened, and as it were ſilver'd over with Mercurial fumes, and was then to reſtore to its native Yellow.

SECT. XXIII.

But let us return to viſible Liquors, and endeavour to prove almoſt *ad Oculum*, as they ſpeak, that their inſenſible parts may be every way agitated, though their motion be but ſeldom viſible to us. Take then what quantity you pleaſe of *Aqua fortis*, and diſſolve in it as much as you pleaſe of ordinary coyn'd Silver, (it not being neceſſary for this Experiment that it be refin'd) and pour the colour-ed ſolution into 12 or 15 times as much fair water, and

then decant or filtrate the mixture, that it may be very clear. If you look upon this Liquor, the parts of it will ſeem to be all of them as perfectly at reſt as thoſe of common water; nor will your Eye be able to diſtinguiſh any Corpuſcles of Silver ſwimming in the Liquor: and yet that there are ſuch metalline Corpuſcles agitated to and fro with and by thoſe of the water will quickly appear, if you immerſe into it a flatted piece of clean Copper, for by that time you have held it two or three minutes of an hour (perhaps not ſo long) in the Liquor, you ſhall ſee the particles of Silver that were roving up and down the Liquor, faſten themſelves in ſuch ſwarms to the Copper-plate, that they will appear in their native hue, and cover it, as it were, with a looſe caſe of Silver, which may be eaſily ſhaken off in the form of a metalline powder: and if ſeveral ſuch Plates be left all night, or for a competent number of hours, in the bottom of the Veſſel, you may the next day find all the particles of Silver that were diſpers'd through the whole body of the Liquor, ſetled upon or about them; the deep blewiſh green tincture you will diſcover in the water proceeding only from ſome little parts of the Copper-plates, and of the Alloy of the Coyn, diſſolv'd by the ſaline particles of the *Aqua fortis*. And I remember, that to compleat the Experiment, I have ſometimes made even theſe fall to the bottom of the Veſſel, by leaving a lump or two of Spelter there for two or three days: for, not only thoſe metalline Corpuſcles that were juſt over or near to the determinate place where I put the Spelter, but alſo all the reſt, into how remote parts ſoever of the Liquor they were diffus'd, did ſetle upon the Spelter, as appear'd both by its increaſe of bulk, and by their leaving the water clear and colourleſs; which plainly ſeems to have proceeded from hence, that the particles of the water were reſtleſly and every way agitated, and ſo by frequently gliding along the ſurface of the Spelter they

muſt

muſt carry thither of the Corpuſcles of Copper mingled with them, ſome at one time and ſome at another, till at length all were brought to it and detain'd there.

SECT. XXIV.

That of the particles of Spirit of Wine, and ſuch like inflammable Liquors drawn from fermented Juyces, though they ſeem to the Eye to be at reſt, a good many do yet move confuſedly and very nimbly, I remember I have long ſince manifeſted by an eaſie and ocular proof which I devis'd about 10 or 12 years ago, when (being yet ſcarce more than a Boy) I firſt began to conſider what Fluidity might be. The Experiment as I writ it down, with all the Circumſtances and Obſervations relating to it, I have not now by me; but having divers times been deſir'd to ſhew it to Learned men (Phyſitians, Mathematicians, and others) I cannot have forgotten thoſe Phænomena of it that are the moſt pertinent to our preſent Subject. Suppoſing then that in pure Spirit of Wine, beſide the aqueous parts that glide ſoftly along each other, there are ſtore of volatile and Spirituous Corpuſcles, whoſe agitation is ſtronger, I let fall (from a pretty height, that it might be broken into ſmall drops by its fall) into any wide-mouth'd glaſs fill'd with this Liquor, (which muſt not be over-dephlegm'd, leſt the Oyl ſink in it) a little common Oyl, or Spirit of Turpentine, which I therefore made choice of, becauſe its tenacity, greater than that of the Chymical Oyls of Spices, makes it that it will neither mingle with Spirit of Wine, nor ſpread it ſelf, as divers other diſtill'd Oyls will, upon the ſurface of it, but keep it ſelf in the form of round drops, whoſe ſhape facilitates their motion. The Oyly drops then ſwimming at the top of the Spirit of Wine, will be, by the diſorderly rovings of the agile parts of it (which hit againſt them little Globes, as the vivous Spirits aſcend to exhale) made to move reſtleſly to and fro in an irregular manner, the drops ſometimes bearing up to

then decant or filtrate the mixture, that it may be very clear. If you look upon this Liquor, the parts of it will ſeem to be all of them as perfectly at reſt as thoſe of common water; nor will your Eye be able to diſtinguiſh any Corpuſcles of Silver ſwimming in the Liquor: and yet that there are ſuch metalline Corpuſcles agitated to and fro with and by thoſe of the water will quickly appear, if you immerſe into it a flatted piece of clean Copper, for by that time you have held it two or three minutes of an hour (perhaps not ſo long) in the Liquor, you ſhall ſee the particles of Silver that were roving up and down the Liquor, faſten themſelves in ſuch ſwarms to the Copper-plate, that they will appear in their native hue, and cover it, as it were, with a looſe caſe of Silver, which may be eaſily ſhaken off in the form of a metalline powder: and if ſeveral ſuch Plates be left all night, or for a competent number of hours, in the bottom of the Veſſel, you may the next day find all the particles of Silver that were diſpers'd through the whole body of the Liquor, ſetled upon or about them; the deep blewiſh green tincture you will diſcover in the water proceeding only from ſome little parts of the Copper-plates, and of the Alloy of the Coyn, diſſolv'd by the ſaline particles of the *Aqua fortis*. And I remember, that to compleat the Experiment, I have ſometimes made even theſe fall to the bottom of the Veſſel, by leaving a lump or two of Spelter there for two or three days: for, not only thoſe metalline Corpuſcles that were juſt over or near to the determinate place where I put the Spelter, but alſo all the reſt, into how remote parts ſoever of the Liquor they were diffus'd, did ſetle upon the Spelter, as appear'd both by its increaſe of bulk, and by their leaving the water clear and colourleſs; which plainly ſeems to have proceeded from hence, that the particles of the water were reſtleſly and every way agitated, and ſo by frequently gliding along the ſurface of the Spelter they

muſt

muſt carry thither of the Corpuſcles of Copper mingled with them, ſome at one time and ſome at another, till at length all were brought to it and detain'd there.

SECT. XXIV.

That of the particles of Spirit of Wine, and ſuch like inflammable Liquors drawn from fermented Juyces, though they ſeem to the Eye to be at reſt, a good many do yet move confuſedly and very nimbly, I remember I have long ſince manifeſted by an eaſie and ocular proof which I devis'd about 10 or 12 years ago, when (being yet ſcarce more than a Boy) I firſt began to conſider what Fluidity might be. The Experiment as I writ it down, with all the Circumſtances and Obſervations relating to it, I have not now by me; but having divers times been deſir'd to ſhew it to Learned men (Phyſitians, Mathematicians, and others) I cannot have forgotten thoſe Phænomena of it that are the moſt pertinent to our preſent Subject. Suppoſing then that in pure Spirit of Wine, beſide the aqueous parts that glide ſoftly along each other, there are ſtore of volatile and Spirituous Corpuſcles, whoſe agitation is ſtronger, I let fall (from a pretty height, that it might be broken into ſmall drops by its fall) into any wide-mouth'd glaſs fill'd with this Liquor, (which muſt not be over dephlegm'd, leſt the Oyl ſink in it) a little common Oyl or Spirit of Turpentine, which I therefore made choice of, becauſe its tenacity, greater than that of the Chymical Oyls of Spices, makes it that it will neither mingle with Spirit of Wine, nor ſpread it ſelf, as divers other diſtill'd Oyls will, upon the ſurface of it, but keep it ſelf in the form of round drops, whoſe ſhape facilitates their motion. The Oyly drops then ſwimming at the top of the Spirit of Wine, will be, by the diſorderly rovings of the agile parts of it (which hit againſt them little Globes, as the vivous Spirits aſcend to exhale) made to move reſtleſly to and fro in an irregular manner, the drops ſometimes bearing up to

one another, as if all or moſt of them were preſently to unite into one body, and then ſuddenly falling off, and continuing to ſhift places with one another, after a manner pleaſant and ſtrange enough to them that never before ſaw the Experiment: and this dance will continue for half an hour or an hour, (or a ſhorter or much longer time, according to the quantity and ſtrength of the Liquor) till the ſpirituous parts being flown away, the drops being no longer impell'd lye at reſt upon the diſſpirited Liquor, as they would upon common water. And whereas the nimble motion of the drops might be ſuſpected to proceed from ſome ſecret contrariety in Nature betwixt the Oyl of Turpentine and Spirit of Wine; beſides that I could eaſily ſhew that thoſe two Liquors have no Antipathy, I not only try'd the Experiment with another inflammable Liquor than Spirit of Wine, but (if I much miſremember not) found, as I expected, that little pieces of chop'd ſtraw (ſuch both being light and not eaſily imbibing moiſture) being gently let fall upon the Spirit of Wine, were in a tumultuous manner carried to & fro upon the ſurface of it; though I am not ſure but that the motion of the Oyly drops may be in part due to ſome partial ſolution made of them by the vivous Spirit, which during that action may tumble them to and fro; not to add that I have by ſome tryals, been tempted to ſuſpect the air may have ſome intereſt in the motion of the drops. However, I have mention'd the recited Experiment, not as if I thought that either it or fugitive Spirit of Wine were fit to teach us the nature of fluid Bodies in general, but to ſhew by an ocular example that there may be a quick and inteſtine motion in ſome parts of a Liquor, notwithſtanding that the unaſſiſted Eye can diſcern no ſuch matter. I ſhall not here relate, how having caus'd to be Hermetically ſeal'd up ſome of theſe Liquors in a glaſs, to try how long the extravagant dance of the drops would laſt, when the

more

more ſpirituous parts of the vinous Liquor could not exhale, my veſſel was ſoon broken without any diſcernable violence. Nor ſhall I now take notice of any of the other Phænomena of our Experiment, partly, becauſe I have elſewhere mention'd moſt of them; and partly, becauſe I think it more pertinent to our preſent Theme, to obſerve that this unſeen agitation of the minute parts will not only hold in light and ſpirituous Liquors: For, that the inſenſible parts even of the heavieſt Liquors themſelves are alſo in actual motion, though many may think it unfit to be believed, will follow from what has been already delivered concerning the nature of fluid bodies, as ſuch; and may be confirm'd by this, that whereas three of the heavieſt Liquors we yet know of, are Quick-ſilver, Oyl of Tartar *per Deliquium*, and Oyl of Vitriol, that firſt-nam'd will even in the cold penetrate into the pores of foliated Gold, and deſtroy the texture of that cloſeſt of Metals; the Liquor alſo of Salt of Tartar will in the cold draw tinctures from ſeveral bodies, and we have endeavour'd to evince the agitation of the parts of Oyl of Vitriol, not only by ſhewing how in the cold it would corrode divers Metals, but by caſting little pieces of Camphire into it, which without the aſſiſtance of the fire were made liquid by it, and appeared like ſo many drops of Oyl. And he that yet doubts, whether the parts of theſe two Oyls (as Chymiſts abuſively call them) how ponderous ſoever they be, are fiercely agitated or no, may probably be ſoon ſatisfied by ſhaking an ounce or two of each of them together, and obſerving the heat, hiſſing, ebullition, and ſparkling, that will ſuddenly enſue upon their being blended.

SECT. XXV.

But here we muſt take notice, that though it belong to the Nature of fluid Bodies, that their parts do eaſily ſhift places; yet that is to be underſtood only as to the parts of the ſame fluid Bodies, as water, or of ſuch differing fluid Bodies

Bodies as are disposd readily to admit each others particles, and mingle together, as we see in Water and Wine. For otherwise, fluid Bodies may be of such differing natures, that when two or more of them are put together, they will not mix, but each retain its own distinct surface; so that in regard of one another, the contiguous Bodies do in some degree emulate each of them the Nature of a consistent Body: for though it cannot be look'd upon as a hard body but a soft, because of the easie Cession of its superficies, yet it does like a compact or consistent body deny to mingle permanently with the contiguous Liquor or other fluid substance. And I somewhat wonder, that *Lucretius* and other Atomists should (at least for ought I remember) over-see this Observation, since it is obvious enough in Oyl, which will not mix with water, but float upon its surface: Not to mention, that Quick-silver will not incorporate with any of the familiar Liquors known to the Ancients. I had almost forgot, that I promis'd at the beginning of this Discourse an Instance concerning Flame, which I will therefore now recite. And it is, That having by an easie preparation of Copper, by the intervention of a little Sal Armoniack, (which I have already taught in another Treatise*) so open'd the Body of that Metal, as to make it inflammable; I took some small grains of this prepar'd Mineral, and put them under the wieck of a strong and actually burning Candle, whereby (as I expected) they were with the melted tallow soon carried up to the bottom of the flame, and by it so kindl'd, that the green (not blew) flame of the cupreous Body did (somewhat to the wonder of the Spectators) burn for a good while (this combustible matter being marvellously lasting) distinct from the yellow flame of the Candle, as if there had been some invisible partition between them. But to return to the unminglable Liquors we were formerly speaking of; the

* *An Historical Dialogue about Flame.*

the cauſe why theſe retain their diſtinct ſurfaces, my preſent task does not oblige me to enquire into: but this I ſhall obſerve in general, that it ſeems to depend very much upon the texture of the particular Liquors, and perhaps too upon the peculiar motions of their minute parts. For I have obſerv'd, that though pure Spirit of Wine and Salt of Tartar, reſolv'd into a Liquor by the moiſture of the Air, will, when put together, retain diſtinct ſurfaces, or preſently regain them if you ſhake the Liquors never ſo ſtrongly together; yet by adding a little fair water to either of them, the texture being thereby alter'd, it will eaſily incorporate with the other. And thus although that (as I noted already) common Spirit or Oyl of Turpentine will not mingle with Spirit of Wine, yet having had the curioſity to make a tryal with Oyl of Turpentine abſtracted skilfully, and with a very gentle fire, (for otherwiſe the Experiment may eaſily miſcarry) from melted or at leaſt well decrepitated Sea-Salt, we found, according to expectation, that though there appeared no viſible alteration in the Oyl, yet we could eaſily by ſhaking confound it with pure Spirit of Wine. Moreover, though lixiviate Liquors and Oyls will not by an ordinary Agitation be permanently joyn'd, yet I have try'd, that by digeſting a good while a ſolution of Salt of Tartar with Oyl of Almonds, I could reduce them to a ſoft Saponary ſubſtance: which Experiment makes ſomewhat more to my preſent purpoſe, than the common practice of Sope-Boylers, becauſe I did not, as they, boyl away the water wherein the lixiviate Salt is diſſolv'd. I might adde alſo, that if you put one part of Quick-ſilver into about four parts of Oyl of Vitriol, you will find (at leaſt if the Experiment proceed always after the ſame manner as it has done with me) that the two Liquors will remain diſtinct whilſt you keep them in the cold, but if by degrees of heat you bring the Oyl of Vitriol to boyl, it will pierce the ſurface of the Quick-ſilver, and

by

by partly incorporating with it, reduce it to a ſubſtance very differing from what it was. But becauſe theſe two laſt Experiments may be with leſs improbability than the two that preceded them refer'd to other cauſes, I ſhall no farther inſiſt on them, but take notice of one thing more concerning the difference of fluid Bodies. And it is this.

SECT. XXVI.

I obſerve, that ſome of them do not only not mingle with others that are contiguous to them, but faſhion the ſurfaces of thoſe others, and reduce them to determinate ſhapes. This I have taken pleaſure to conſider in ſome Chymical Liquors, which I have purpoſely put together; for Inſtance, having pour'd Spirit of Wine upon Oyl of Tartar *per Deliquium*, (as Chymiſts call it) I found that the ſuperficies wherein they touch'd each other was flat, or (as to ſenſe) parallel to the Horizon. But if this were done in a very ſlender or narrow glaſs, with the mouth unſtop'd, though the lower ſurface of the Spirit of Wine which touch'd the other Liquor appear'd very level; yet the upper ſuperficies, which was contiguous to the Air, was manifeſtly very concave. And if to theſe two Liquors I did in a broader glaſs pour Oyl of Almonds, that Oyl would ſink to the bottom of the Spirit of Wine, (that being well rectifi'd) and floating upon the Oyl of Tartar, would ſeparate the two Liquors, and both above and below retain a flat or level ſurface. But if inſtead of Oyl of Almonds, or another expreſs'd Oyl, I dropt into pure Spirit of Wine, ſwimming upon Oyl of Tartar, ſome common Oyl of Turpentine; the Oyl would gather into parcels, (ſome of the bigneſs of hail-ſhot, ſome as big as ſmall Piſtol-bullets, and ſome of other ſizes) which in caſe they did ſwim in the Spirit of Wine, and touch'd neither of its ſurfaces, ſeem'd globulous, and continu'd ſo (the glaſſes being ſtop'd) for many hours: But in caſe they emerg'd to the upper part of the Spirit of Wine, as much of them as lay immers'd

immers'd in the Spirit (which was by far the greateſt part of them) appear'd round, and continu'd ſo long as I pleas'd; the upper parts only of theſe little globes appearing to have the ſame ſurface with the Spirit of Wine. And I further obſerv'd, that ſome ſmall drops would as it were reſt conſtantly upon the ſuperficies of the Oyl of Tartar, touching it but as it were in a point, and continuing to the Eye Spherical; though the ſurface of the Liquor were purpoſely now and then ſomewhat ſhaken. But that which I took ſpecial notice of was, that having (upon deſign) into pure Spirit of Wine (for upon common Spirit Oyl will ſwim) let fall ſome great drops of Oyl of Turpentine, they did at firſt ſink to the bottom of it, and lie upon the ſurface of the Oyl of Tartar almoſt like Hemiſpheres, whoſe convex part was all above the Oyl of Tartar; but after a while they were, as I expected, preſs'd on all ſides and faſhion'd into round Bodies (yet a little more protuberant at the ſides than the top) which ſeem'd ſcarce to touch the ſurface of the Oyl of Tartar on which they lean'd. Diverſe other Obſervations of this kind were afforded me by ſome peculiar mixtures that I made of Chymical Liquors: But not having the leiſure to ſet them down, much leſs to enquire into their cauſes, I ſhould ſcarce have mention'd what I have already deliver'd (eſpecially ſince we found that a light variation of Circumſtances would often alter the event of ſuch tryals, which we have therefore ſet down barely hiſtorically) but that finding that drops of Water, Quick-ſilver, and other fluid Bodies, ſeem'd to be faſhion'd into a round figure by that every way almoſt equal preſſure of the ambient Air; and having likewiſe try'd, that Quick-ſilver ſuſpended in the Air (as it may eaſily be, if the Torricellian Experiment being made in a tube exceeding ſlender, ſome Air be afterwards dexterouſly let in to divide the long Mercurial Cylinder into diverſe ſhort ones) has both at the top and

bottom, where it is expos'd to the Action of the Air, a very protuberant ſurface; finding, I ſay, theſe Effects of fluid Bodies upon one another, I thought it not amiſs to intimate, how ſome Experiments might be made that may poſſibly facilitate the giving an Account of the figuration of ſome of the more conſiderable fluid Bodies, which, as we noted already, make up much the greater part of the Univerſe: eſpecially, ſince I could eaſily enough make it probable, that ſuch ſteams of the terreſtrial Globe as may well be ſuppos'd to be the chief Ingredients of our Atmoſphere, may like a Liquor retain a ſuperficies diſtinct from that of the ambient and contiguous Body. And ſince we are ſpeaking of the diſtinct ſurface of fluids, the occaſion invites me to add an Experiment, which, though apt to miſcarry upon the account of unheeded Circumſtances, has yet, when all things were rightly order'd, ſucceeded very well: I will, I ſay, ſubjoin it here, becauſe it ſhews a way of dividing in a trice, a Liquor Tranſparent, and, as to ſenſe, Homogeneous into two very differing Liquors, the one Diaphanous, and the other Opacous, which will not mingle. The Experiment is this; Diſſolve one Ounce of clean common Quick-ſilver in about two Ounces of pure *Aqua fortis*, ſo that the Solution be clear and total, then whilſt it is yet warm, pour into it by degrees, leſt they boyl over, half an Ounce or one Ounce of Filings of Lead, and if no Error, nor ill Accident have interven'd, the Lead will be in a trice præcipitated into a white Powder, and the Mercury reduc'd into a Maſs (if I may ſo ſpeak) of running Quick-ſilver, over which the remaining part of the *Aqua fortis* will ſwim, whereby we may ſee that Liquors being reduc'd into very minute parts, may mingle very well, the Corpuſcles of the one ſupporting in that ſtate thoſe of the other, though in greater Bulk, eſpecially the Texture of one being ſomewhat varied, they will retain diſtinct Surfaces. *N.B.* Note, that when the Operation

ration ſucceeds not well, the Mercury need not, for all that, be loſt, but may (in great part at leaſt) be recover'd by freeing the Præcipitated matter from the reſt by filtration, and then diligently grinding the white Præcipitate with Water, by which means, the Mercury will little by little be gathered into drops. And though this be far from being the true Mercury of Lead, as I may elſewhere ſhew you; yet ſome Inducements, not here to be named, incline me to look upon it, as ſomewhat differing from common Mercury, and fitter than it for certain Chymical uſes.

SECT. XXVII.

And here I ſhould paſs on to the Conſideration of Firmneſs; but that when a while ago I diſcours'd of the Agitation of the Corpuſcles that compoſe Oyl of Tartar and Oyl of Vitriol, I forgot to add, that not only in fluid bodies, but in ſome alſo of thoſe that are conſiſtent, there may perhaps be more motion in the inſenſible parts than our ſenſes diſcern, or we are wont to imagine: eſpecially in thoſe bodies, which having been once endowed with life, are, though not fluid, yet either ſoft, or at leaſt not perfectly hard. I have more than once taken pleaſure to look upon an heap of ſwarming Bees, for though they make not up a liquid but coherent body, which may be turn'd upſide down without loſing its coherence, and which being beheld at a diſtance, ſeems to be one entire maſs or body; yet it is evident to him that looks at them near enough, that the particular Bees that ſwarm have moſt of them their diſtinct and peculiar motions, and that yet theſe motions of the particular Bees deſtroy not the coherency of the heap; becauſe that when one of the more innermoſt Bees removes, as ſhe lets go her hold from thoſe that ſhe reſted on before, and goes away from thoſe that reſted on her, ſo ſhe meets with others on which ſhe may ſet her feet, and comes under others that in like man-

ner ſet their feet on her, and ſo by this viciſſitude of mutual ſupports their coherence and their removes are made compatible; and if inſtead of Bees, the ſwarm conſiſted of extreamly little flies, their particular motions would perhaps be inconſpicuous. And that ſome ſuch thing may happen in the conſiſtent bodies we have been ſpeaking of, ſeems probable from hence, that in wainſcot and other hard wood, we often ſee little heaps of duſt produc'd in them by putrefaction; and not only in Cheeſe we many times ſee multitudes of mites ſtart up, but in Apples and other Fruits we oftentimes find Magots, though the skin be whole, (which could not be unleſs the parts of the matter were variouſly tranſpos'd, (that is, put into a local motion) and connected after a manner ſuitable to the Nature of the inſect to be produc'd:) And by the growth of bones in the bodies of perfecter Animals, as well in reſpect of the internal cavity, where the marrow lodges, as of the external ſurface, as alſo by the growth of the ſhells of Oyſters and Snails, (though cold Animals) from a ſize inconſiderable at firſt, in regard of what is afterwards attain'd to, and by ſome other reſembling particulars, it ſeems that the ſmall particles that conſtitute even the ſolid parts of Animals are not, whilſt the Creature lives, (or at leaſt whilſt it grows) altogether exempt from ſome (though ſlow and inſenſible) local motion. And I remember, that it has by a very diligent obſerver been affirmed to me, that he ſaw ſeveral pieces of Gum ſwet out of an old wainſcot of above twenty years ſtanding. Which I the leſs wonder at, becauſe I have ſeveral times ſeen viſcous Exſudations diſcloſe themſelves like drops of Turpentine upon Deal-boards, which had been made uſe of about Buildings. (But of this ſubject more (perhaps) elſewhere.)

SECT. XXVIII.

After we have hitherto diſcours'd of Fluidity as conſiderd

der'd in diſtinct Bodies, we might properly enough ſay here ſomething of what furtherance or hinderance in reſpect of Fluidity one Body may receive by being mingl'd with another. But the conſideration of thoſe changes of Conſiſtence which may be produc'd by Mixture, is a Subject that we ſhall have ſuch frequent occaſions to treat of in what we are to deliver about Firmneſs, that we ſhall now only give this general Admonition, That 'tis not ſo ſafe as one would think, to fore-tell the conſiſtence of a mixture of two or more Bodies, from the bare conſideration of the conſiſtence of thoſe Bodies whereof it is to be compounded. And that we might at once both manifeſt this, and inſinuate what Judgment ſhould be made of what is ſaid by ſo many Chymiſts and others, who without Limitation teach, That the Addition of Salts to metalline and mineral Bodies does much facilitate their fuſion, I remember I purpoſely made and employ'd this Experiment.

We diſſolv'd crude Copper in a due quantity of Spirit of Nitre, and by Evaporation reduc'd the Solution to a kind of Vitriol of a lovely colour. We alſo corroded with two parts of Spirit of Nitre one of good Tin, and ſuffer'd the mixture to reduce it ſelf (as it eaſily did) to a ſubſtance almoſt like Meal. Of this mixture we put a parcel into a Crucible, and ſuffer'd it to grow (and for a pretty while to continue) red hot: Nay, we put ſome of it upon a quick coal, and excited the heat by frequently blowing the fire, without finding that this metalline meal did at all melt, though Salt-Petre be a fuſible Salt, and Tin it ſelf be of exceeding eaſie fuſion. Whereas, although Copper be a metal which is much harder to melt, not only than Tin and Lead, but even than Silver, (as thoſe well know that mix Silver with Copper to make a fuſible mixture to ſoder upon Copper and Braſs) yet was this metal, that will endure a long and ſtrong Ignition by being

being joyn'd *per Minima* with the ſame kind of Nitrous Corpuſcles, that had ſo contrary an effect upon the Tin, ſo ſtrangely diſpos'd to fuſion, that the Vitriol would melt with as ſmall a heat as that of ones hand. Nay, we have made ſuch a Vitriol either with Spirit of Nitre or with (what differ'd little from it) a certain *Aqua fortis* as would even in Winter be made to ſtand melted for divers hours together, by the languid warmth of the Sun, though ſhining on it but through a window, where it alſo ſtood but in an unſtop'd Glaſs. So fit it is that we conſider as well the new Texture that mingl'd Bodies obtain by the aſſociation of their particles, (whoſe ſize and ſhape, and perhaps motion may be thereby much alter'd) as the conſiſtence of the particular Bodies before their being mixt.

THE

THE HISTORY OF *Fluidity* and *Firmneſſe*.

The Second Part.

Of Firmnesse.

IT is ſufficiently known that the Chymiſts aſcribe the Firmneſs and Hardneſs of Bodies to Salt, and teach that the Saline Ingredient of them is the Principle of Coagulation in them, and the cauſe of their Compactneſs and Solidity. But though this opinion of the Chymiſts be embra'cd by ſo many modern Philoſophers and Phyſitians, that ſome may think it ſuperfluous to make enquiry after other Cauſes, yet others (to whom the Explications of Chymiſts ſeem not always ſo much as Intelligible) will upon the very account of the Receivedneſs of the propos'd Opinion, think it rather worthy to be examined than to be acquieſc'd in. However, without making it our buſineſs, either to ſide with, or oppoſe any Sect of Naturaliſts, we will apply our ſelves a while to conſider the thing it ſelf in proſecution of the Deſign already begun. And having in the fore-going

Part

Part of our little Hiſtory taken a general view of Fluidity; we will now proceed to try what Light it will afford us to diſcover the nature of Firmneſs or Compactneſs.

And ſince fluidneſs and ſtability being contrary qualities, are to be apprehended under contrary notions, we may conceive that the firmneſs or ſtability of a body conſiſts principally in this, that the particles that compoſe it, beſides that they are moſt commonly ſomewhat Groſs, either do ſo reſt or are ſo intangled between themſelves, that there is among them a mutual coheſion whereby they are rendred unapt to flow or diffuſe themſelves every way, and conſequently to be, without violence, bounded and figur'd by other ſurfaces than thoſe which their connexion makes themſelves conſtitute.

In this rude Deſcription of Firmneſs we have intimated Three principal cauſes of it, namely the Groſſneſs, the quiet Contact, and the Implication of the component parts.

The firſt is Groſſneſs of Parts, of which we have in effect almoſt ſufficiently diſcours'd already: for ſince treating of Fluidity, we manifeſted at large how conducive ſmalneſs of Parts was to that Quality; 'tis eaſie to deduce that Groſſneſs of parts in a Body muſt commonly be a great diſpoſition to its being Firm. And bigger Corpuſcles being *cæteris paribus* more difficult than leſſer to be put into motion, when they are once at reſt, it is obvious that a Body conſiſting of ſuch Particles is leſs diſpos'd to become fluid, and conſequently more apt to continue firm, than if its component parts were ſmaller, and thereby more eaſie to be diſplac'd. But when I ſpeak of the Groſſneſs of Corpuſcles, I pretend not to determine whether or no Body or Matter be ſo perpetually diviſible, that there is no aſſignable portion of matter ſo minute that it may not at leaſt Mentally (to borrow a School term) be further

further divided into ſtill leſſer and leſſer parts: For allowing this indefinite Diviſibility of corporeal ſubſtance, 'tis plain that it may in ſome ſenſe be averr'd, that there are no firm Bodies whoſe Parts are not extreamly minute. But I underſtand by the groſs parts I here ſpeak of, ſuch Corpuſcles as actually convening to conſtitute a Body, are ſcarce diſſipable or diviſible into leſſer by the Agitation of the ambient Air or Æther, or by the other cauſes of the Fluidity of Bodies.

It is alſo to be noted, that when I ſpake of the fitneſs of groſſer Corpuſcles to make a firm Body, I added, *Cæteris paribus*, becauſe it may happen that the breaking of the ſmall parts of a Body into minute Particles may make them but the fitter to contribute to the Firmneſs of the Body they belong to: For the parts of the divided Corpuſcles may by their comminution acquire a new, and perhaps a more irregular ſhape than before, upon whoſe account they may be more diſpos'd to be entangled among the neighbouring Particles, or may be better fitted to get into and fill the pores of ſome kind of Bodies. And in in ſuch little Particles, not only the minuteneſs may make them lie cloſer together, and conſequently the better exclude the Air: but the greatneſs of the ſurface in proportion to the bulk of the matter may perhaps in ſome Caſes occaſion a fuller contact, and ſo facilitate the conſtitution of a very firm Body, in caſe theſe minute parts (whoſe intervals (if they intercept any) need not be other than very ſmall) ſhall be placed and diſpoſed to the beſt advantage for a full contact of one another. But as I ſaid a while before, from what we have already delivered (concerning the ſize of parts, when we treated of Fluidity) it may eaſily be underſtood how much the magnitude of them may conduce to Firmneſs; and therefore we will preſently paſs on to the mention of the two other things to be conſidered in reference to conſiſtent Bodies. Where-

of the one is the bare Reſt of the ſmall and contiguous parts that make up the firm Body; and the other the intricate Texture of ſuch parts in the body they make up. And either of theſe two ſeems alone ſufficient to render a body ſtable: though Nature do perhaps oftentimes make ſome (though not equal) uſe of both, to faſten the parts of the ſame body more firmly together.

Of the former of theſe Cauſes, I am inform'd that the juſtly famous Monſieur *Des Cartes* has alſo taken notice, but without adding proof from Experiments, or Obſervations. But it ſeems to have been either over-look'd, or, (as incongruous to their Hypotheſis of the innate motion of Atoms) rejected by the old Atomiſts, and by *Lucretius*, who takes notice (that I remember) only of the latter: for though they did of old make mention of the ſudden divulſion of two flat and ſolid Bodies, yet they employ that Obſervation but to prove a Vacuum, (without otherwiſe taking notice, that I have met with, of thoſe things that are moſt material in ſuch Obſervations to our preſent purpoſe, and without deducing thence what we ſhall endeavour to do in order to the explication of the cauſes of Firmneſs.) Upon what account then ſoever the Atomiſts have omitted to reckon for a cauſe of Firmneſs that which we have newly been ſpeaking of; yet (as we obſerved above) If two bodies be once at reſt againſt one another, it ſeems conſonant to the Catholick Laws of Nature, that they ſhould continue in that ſtate of reſt, till ſome force capable to over-power their reſiſtance puts them out of it. And whatever may be ſaid of the unlooſable mobility of Atoms ſtrictly ſo taken, yet that diverſe parts of Matter may compoſe bodies that need no other Cement to unite them than the juxta-poſition which we here preſuppoſe, and the reſting together of their parts, whereby the Air and other fluid Bodies that might diſſociate them are excluded, I have been inclined to think

think by what I have obſerved in grinding of Glaſſes: for ſometimes the convex ſurface of one body being ground upon the concave ſurface of another, the two ſurfaces will happen to be ſo cloſely and exactly fitted to one another, (their immediate contact in all their parts, or at leaſt in innumerable of them, hindering the intercurrence of the Air) that a man is not able without breaking one or both of them to pull them directly aſunder; but if you will ſever them, you muſt be fain to make one of them to ſlip along the ſurface of the other: which makes the Glaſs-Grinders often complain of the trouble they meet with in ſeparating ſuch bodies. Nay, if you lay two flat Glaſſes ground very true and well poliſhed upon one another, ſo that their ſurfaces may almoſt every where touch each other, (to which it will be requiſite to rub them a little one upon another, for the better excluſion of the Air) you may by lifting up the uppermoſt, lift up the lowermoſt (though perhaps, as we have often try'd, two or three times bigger) with it, as if the two Plates of Glaſs made but one body. Nay, we have divers times taken up four or five pieces of Glaſs at once, laid and preſt thus one upon another, and might perchance have taken up a greater number, if we had had more of them at hand. And tryal has alſo informed us, that if you hold a Looking-Glaſs very level with the unfoliated ſide downward, and rub a little againſt it a piece of other very flat and very ſmooth Glaſs, you may eaſily by that way only, faſten them to one another; ſo that the lowermoſt Glaſs, though large, will hang between the uppermoſt and the ground, to the wonder of thoſe that behold it, and can diſcern nor imagine nothing capable to keep it from falling: and by the ſame way (as we ſhall recite anon) we have often made one conſiderably thick piece of Marble take and hold up another, having purpoſely cauſed their flat ſurfaces to be carefully ground and po-

liſh'd, without which the Experiment will not ſucceed. Nor is it requiſite that the glaſſes be flat to make them adhere very cloſely together, provided their immediate contact be made according to a large ſurface: for to what we have already mention'd concerning the cohering of convex and concave Bodies, we may adde, that having purpoſely appli'd a long glaſs-ſtopple of an almoſt conical figure, and well ground, to the mouth of a thick quart Bottle, whoſe neck was made long and of a figure convenient to receive the ſtopple, and ground within fit for it, we found that theſe two glaſs bodies touching one another in a multitude of parts, did adhere together ſo cloſely, that when the ſtopple was carefully put in, we could eaſily, and divers times one after another, liſt up the bottle, though there was by our gueſs above a pound of Liquor in it. Unleſs we ſuppoſe, without much probability, that becauſe 'tis found, that moving them to and fro upon one another, and preſſing down the ſtopple, promotes their ſticking, their adheſion may be in part aſcrib'd either to ſome Elaſtical motion in the parts of the preſſed glaſs, or to the exquiſite adaptation of the almoſt numberleſs, though very ſmall, aſperities of the one, to the as numerous little cavities of the other; whereby the ſurfaces do lock in with one another, or are as it were claſp'd together. For as poliſh'd as the ſurfaces may appear to ſenſe, we muſt not deny that there may be ſuch inequalities in them, ſince being wont to be poliſh'd with Putty or ſome ſuch powder, or heap of grating and irregularly ſhap'd Corpuſcles, they muſt needs make ſtore of little furrows, and ridges, and other Aſperities on them. But to inſiſt on theſe Conjectures were to digreſs.

Yet here we muſt not decline taking notice, that, at leaſt here below, the ſticking together of ſuch bodies as are of ſenſible Bulk, and whoſe ſmooth ſurfaces immediately touch one another, may poſſibly not ſo much proceed

ceed from this, that their parts, as we formerly obſerv'd, are at reſt among themſelves, and by their immediate contact do make up as it were but one body; as from the preſſure of the Atmoſphere, proceeding partly from the weight of the ambient Air, (mixt with the Effluviums of the terreſtrial Globe) and partly from a kind of Spring, by vertue of which the Air continually preſſes upon the bodies contiguous to it, though through accuſtomance & negligence, & perhaps ſome other cauſes not here to be inſiſted on, we neither feel it in our own bodies, nor take notice of it in others. Now this preſſure of the Air every way being ſuppos'd, I think the adhering of the ſmooth bodies we ſpeak of (for we ſuppoſe them far greater than the particles of the Air) to one another may probably enough be aſcrib'd to this, That when, for inſtance, the ſmooth ſurfaces of two pieces of Glaſs do ſo exquiſitely touch one another, that none of the ambient Air is either intercepted or admitted between them, then the undermoſt glaſs will ſuffer no preſſure on that ſide which touches the uppermoſt; the parts of the uppermoſt glaſs having no ſenſible ſpring in them (ſo that they can only Reſiſt, but not Repell the other:) but that ſide of the undermoſt Glaſs which is expos'd to the Air will be preſs'd upon thereby; and there being, as we ſaid, no Elaſtical preſſure on the other ſide of the glaſs to balance this, it is not to be wonder'd at that the inferiour glaſs ſhould not fall off from the other, in regard the weight that would carry it downwards is much too ſmall to overcome that force of the Air that thruſts it againſt the uppermoſt glaſs: As if one ſhould with his hand thruſt a plate of Iron broad-wiſe againſt the flat cieling of his Chamber, the Iron would not fall as long as the force of the hand perſeveres to preſs againſt it. Nor is it material, that in our Caſe the preſſure of the Atmoſphere is ſuppos'd to force the lowermoſt glaſs upwards: For if we ſuppoſe the Air to conſiſt of innume-

numerable little ſprings (as it were) bearing upon and ſupporting one another, and whereof the lowermoſt are bent by the Weight of all that are incumbent on them, it will be eaſie to conceive that neer the ſurface of the Earth, (about which the Air muſt diffuſe it ſelf by reaſon of the Gravity of its ſmall parts, and the Reſiſtance of the Earth againſt their Deſcent) it may preſs almoſt equally every way, and by a kind of Recoyl (though not properly ſo call'd) from the Terreſtrial Globe upwards, may ſtrongly preſs any body upon which it can bear, againſt any other which has no ſuch Elaſtical power to repel from it a body ſo preſs'd againſt it.

This Difficulty being thus diſpatch'd, we ſhall proceed by two or three particulars to confirm our Conjecture at the Cauſe why ſmooth Bodies ſtick together upon bare juxta-poſition or contact. And firſt I obſerve, that if a piece of flat glaſs be, as we formerly mention'd, appended to a Looking-glaſs held with the unfoliated ſide downwards, parallel to the Horizon, though the adhering glaſs will not drop down, yet it will very eaſily be mov'd any way along the level ſurface; and if by inclining the Looking-glaſs any way, you deprive it of its former Level, the ſmaller glaſs will eaſily ſlide downwards upon the ſurface of the greater. Of which the Reaſon ſeems to be, partly that the Gravity as ſuch of the lower glaſs does not conſiderably reſiſt the horizontal motion of it, but only the motion upwards, whereby it muſt recede from the Centre of heavy Bodies, as might, if need were, be probably deduc'd from divers Inſtances obvious enough; and partly, or rather chiefly, that to the edges of the glaſs the Ambient Air is contiguous as well on the one ſide as on the other, and ſo the preſſure of the Air being equal on every part of the edges, the gravity of the ſmaller glaſs is not hinder'd by the Air (which can as faſt ſucceed on one ſide, as 'tis diſplaced on the other) from making it

ſlide

ſlide down the ſhelving ſurface of the greater Glaſs, whereas of the broad and flat ſides of the lowermoſt glaſs the one is, as we ſaid, preſs'd by the ſpring of the Air, whilſt the other ſuffers no ſuch preſſure from the Looking-glaſs, to which it was apply'd. And ſo, if you take a ſmall open-mouth'd glaſs, and plunge it into a Veſſel full of Quick-ſilver with the mouth upward, that the Quick-ſilver may fill it without leaving any Air in it, and if then, whilſt it is under the Quick-ſilver, you turn the mouth downwards, and ſo keeping it upright, lift it up till the mouth be almoſt come to the top of the Mercury; you ſhall perceive that the glaſs will remain almoſt full of Quick-ſilver in the Veſſel: And this will continue ſo, though you incline the glaſs this way or that way, provided you keep the mouth of it within the Mercury. And this Experiment, though more noble when try'd with Quick-ſilver, will ſucceed alſo when tryed, as it may more eaſily be, with water. Of which the Reaſon ſeems to be, that the glaſs hinders the Quick-ſilver in it from the preſſure of the incumbent air, whereas the Quick-ſilver in the Veſſel being expos'd to it, muſt by it neceſſarily be forced up againſt the ſurface of the inverted bottom of the glaſs, where it meets no Elaſtical power to repell it downwards. For, that it is not Natures Abhorrency of a Vacuum, that keeps the Quick-ſilver from deſcending till ſome air can come to ſucceed in its room, the famous Experiment invented by *Torricellius*, and found true by many others, and our ſelves, touching the deſcent of Quick-ſilver in any Tube of above two foot and a half long, ſeems clearly to evince. And to confirm what we have ſaid, and ſhew withall, that it is not ſo much the Contact of Bodies according to a large ſurface, as that Contact is conſidered in it ſelf, as by reaſon of its being ordinarily requiſite to the excluſion of Air, that at leaſt here below keeps bodies from falling aſunder; I ſhall relate

relate, that having by a certain Artifice out of a large glaſs (with a narrow mouth) caus'd a certain quantity of air to be ſuck'd, we found that by immediately applying a Book (which then chanc'd to lie at hand) to the Orifice of the Veſſel, the Book was readily lifted up and ſuſtein'd in the air as long as we pleas'd, though the ſurface of the ſuſpended Body could be touch'd, as is evident, but by the Ring which incircl'd the Orifice of the Veſſel, and though the weight taken up (beſides that it was inconveniently ſhap'd for ſuch a trial, which would probably have ſucceeded as well with a much greater weight, if we had had one fitly ſhap'd at hand) exceeded twenty Ounces *. Of which event the Cauſe ſeems plainly to be this, that by reaſon of the Exſuction of ſome air out of the glaſs, the Elaſtical power of the remaining Air was very much debilitated in compariſon of the unweaken'd Preſſure of the External air, which being able to preſs the Book againſt the Orifice of the Veſſel with greater ſtrength than the internal air can reſiſt, thereby it comes to paſs, that the whole Orifice of the Veſſel, though there be but part of it of ſolid body, does yet on this occaſion perform in ſome meaſure the part of an entire ſurface exactly ſmooth.

* *Much more conſiderable Inſtances of this nature may be met with in the Author's* New Phyſico Mechanical Experiments.

It may be conſider'd alſo (to adde that upon the by) whether upon the Principle lately explicated may not in ſome meaſure depend the ſolidity of glaſs. For though its parts ſeem very little or not at all branched or interwoven one within another, and appear very ſmooth and ſlippery, yet ſince the fire that brought them to fuſion, and conſequently to be fluid, may well be ſupposd to have ſub-divided and reduc'd them into very ſmall Particles, and to have thereby aſſiſted them to exclude the air from betwixt them, it may ſeem that it needs not much

be

be wonder'd at, if the immediate contact of ſuch ſmall and ſmooth Corpuſcles ſuffice to make them hold together; for that their union is ſtrict enough to keep out the air, may appear from this, that thoſe that blow glaſſes, and thoſe that diſtill in them, find not the air can traverſe the pores even of heated glaſs; and as for any more ſubtil matter, we ſee by the free paſſage of Light and Heat, or, to ſpeak more warily, of magnetical Effluvia through glaſs, without injuring its texture, that ſuch matter but moderately mov'd will not hinder the little ſolid parts from cleaving together. And on this occaſion it might be conſider'd, how much the juxta-poſition of their Corpuſcles crowded together by fuſion may contribute to the conſiſtence and brittleneſs of Salt-Petre, and diverſe other bodies, which may from an incoherent powder be readily turn'd into one Maſs; as alſo how far the ſticking together (for I ſpeak not of the figures compos'd by them) of the ſmall parts of hanging drops of water, and ſuch other Liquors as are not thought to conſiſt of Corpuſcles hooked or branch'd, may be aſcrib'd to the contact of their ſmall parts, and the excluſion of air. Theſe, I ſay, and ſome other ſuch things might be here conſider'd, but that we are forbidden to examine them particularly, and eſpecially what has been repreſented touching the ſolidity of glaſs, (which we ſuſpect another cauſe may have a great Intereſt in) by our haſte, which calls us to the remaining part of our Diſcourſe.

Though then it be hence (to omit other proofs elſewhere mention'd) ſufficiently manifeſt, that the Air has a ſpring, and that a ſtrong one, yet there appears no great neceſſity of having recourſe to it for the giving an Account why the two ſmooth glaſſes above mention'd were able to adhere ſo cloſely to each other: For a probable Reaſon of the ſame Phænomenon may be render'd by the preſſure of the Air conſider'd as a weight. And firſt,

we muſt recall to mind what we (a little above) ſaid of the Recoyling, (or Rebounding of the Preſſure) of a Cylinder of Air from the Earth, to the ſuſpended piece of glaſs, proceeding from this, that the fluid Air, which is not without ſome Gravity, being hinder'd by the reſiſting ſurface of the terreſtrial Globe to fall lower, muſt diffuſe it ſelf, and conſequently preſs as well upwards as any other way. Next, we may conſider, that when the ſurfaces of two flat Bodies of any notable (and for example of equal) breadth do immediately touch each other, and lye both of them level with the Horizon, and one of them directly over the other; in this caſe, I ſay, ſince the Air cannot move in an inſtant from the edges to the middle of the two ſurfaces that lye upon each other, the lowermoſt cannot be drawn away downwards in a perpendicular line from the uppermoſt, but that by reaſon of the ſtiffneſs and contact of the two Bodies, it muſt neceſſarily happen that at the inſtant of their ſeparation, ſhould it be effected, the lowermoſt glaſs will be preſs'd upon by the whole (Crooked) Pillar of Air, ſuppos'd to reach from the top of the Atmoſphere, and to have for Baſis the ſuperficies of the undermoſt glaſs. For at that inſtant, the Air having not time to get in between the two glaſſes, there is nothing between them during that inſtant to reſiſt the preſſure of that Air which bears againſt the lower ſuperficies of that undermoſt glaſs, and conſequently ſuch a revulſion of the lower glaſs cannot be effected but by a weight or force capable to ſurmount the power of the weight of the abovemention'd Cylinder of the Atmoſphere; and this, as I ſaid, becauſe that by reaſon of the ſudden ſeparation, the upper ſurface of the glaſs has not any air contiguous to it, which, were it there, would (according to the nature of Fluid and ſpringy Bodies) preſs as much againſt the upper ſurface of the glaſs, as the Pillar of the Atmoſphere againſt the lower, and conſequently

quently ſuſtain that Endeavour of the Air againſt the lower ſide of the glaſs, which in our propos'd caſe muſt be ſurmounted by the weight or force employ'd to draw down the lower glaſs. And hence we may underſtand (to adde that upon the by) That it is not neceſſary that the contiguous ſurfaces of the two flat glaſſes we have been treating of, be parallel'd to the Horizon: For if you ſhould hold them perpendicular to it, their divulſion would not ceaſe to be difficult, provided it were attempted to be made by ſuddenly pulling one of the broad ſurfaces from the other in a level line, and not by making one of the ſurfaces ſlide upon the other; for in the former caſe, the ſeparation of the contiguous Bodies will be hinder'd by the weight or preſſure of the lateral Air (if I may ſo ſpeak) that bears againſt the broad ſides of the glaſſes contiguous to it. But whereas in theſe caſes we ſuppoſe the ſuperficies of the two glaſſes to be ſo exactly flat and ſmooth, that no Air at all can come between them; Experience has inform'd us, that it is extreamly difficult, if at all poſſible, to procure from our ordinary Tradeſmen either Glaſſes or Marbles, ſo much as approaching ſuch an Exquiſiteneſs: For we could very hardly get either experienc'd Stone-cutters, or Perſons ſkill'd at grinding of glaſſes, to make us a pair of round Marbles, though of an inch or two only in Diameter, that would for ſo much as two or three minutes hold up one another in the Air by contact, though they would eaſily enough take up each other, if the uppermoſt were drawn up nimbly, before the Air could have leiſure to inſinuate it ſelf betwixt them.

But this notwithſtanding, we endeavour'd by the following Expedient, not only to manifeſt that the Power or Preſſure of the Air is in theſe Experiments very great, but alſo to make ſome Eſtimate (though but an imperfect one) how great that Power is.

Having then provided a pair of Marbles of an inch and half in Diameter, and as flat and ſmooth as we could get, and having conſider'd, that as 'twas the getting in of the Air between them that (for the reaſon above declar'd) hinder'd them from ſticking ſtrongly together; ſo the Acceſs afforded to the Air was for the moſt part due to that ſcarcely evitable roughneſs or inequality of their ſurfaces that remain'd in ſpight of the Poliſh: conſidering theſe things, I ſay, we ſuppos'd that the intruſion of the Air might be for ſome while prevented by wetting the ſurfaces to be joyn'd with pure Spirit of Wine; and that yet this Liquor, that ſeems the freeſt that we know of from tenacity, would not otherwiſe than by keeping out the air prove a Cement to faſten the ſtones together. But becauſe the eaſie ſeparation of ſuch ſmooth Bodies as adhere but by contact does in great part (as we formerly noted) proceed from this, That whereas it is very difficult to hold ſuch Bodies exactly level for any conſiderable ſpace of time, and yet the leaſt Inclination any way gives the lower Body opportunity to ſlide off; becauſe of this, I ſay, we reſolv'd in the firſt place to ſee what could be done by faſtening to the upper Marble certain Wires and a Button, in ſuch manner as that the lower Marble, when it was joyn'd, might freely fall directly down, but not ſlip much aſide, being hinder'd by the Wire. And in purſuit of this we found, that not only the dry Marbles could be made to take up and hold up one another, but that once by drawing up the upper Marble nimbly, we could take up (but not keep up for any time) together with the lower Marble, a Scale, and in it a pound weight of 16 Ounces Troy.

After this we moiſten'd the ſurfaces of the Marbles with ſuch pure Alkalizate Spirit of Wine as we elſewhere teach to make, which was ſo thin and ſubtil, that not only we burn'd ſome of it before we would employ it about

about this Experiment in a Silver Spoon, without leaving ſo much as any ſign of Phlegm behind; but it would (in the open air) almoſt in a moment fly away from the ſurface of the Marble anointed with it, and leave it dry and gloſſy. The Marbles being ſkilfully wetted, and kept by the above-mention'd wires from ſlipping aſide, we caſt into a Scale faſten'd to the lower of them diverſe weights at ſeveral times, and by nimbly pulling up the higher ſtone, try'd many times how much we could draw up with the lower, and did ſometimes take up above an hundred Ounces, and once an hundred thirty two Ounces Troy, beſides the Scale that contain'd them, and the Marble it ſelf, the Diameter of whoſe ſmooth ſurface was by meaſure but about an inch and two thirds.

But here I muſt take notice, both in relation to this and the following Experiments to be ſet down concerning ſmooth Marbles, that we never yet found any ſort of Experiments, wherein ſuch ſlight variations of circumſtances could ſo much defeat our Endeavours; which we therefore mention, that in caſe ſuch Experiments be try'd again, it may be thought the leſs ſtrange, if others be not able to do as much at the firſt and ſecond, or perhaps the tenth or twentieth tryal, as we did after much Practice had made us expert in this nice Experiment, and ſuggeſted to us divers facilitating Circumſtances, which could not here in few words be particularly ſet down.

And now, becauſe we perceiv'd that the Spirit of Wine was too fugitive and ſubtil a Liquor for our purpoſe, we ſuppos'd that Oyl, as it would better fill up the little cavities of the Stones, ſo it would more exactly keep out the air, and leſs eaſily vaniſh into it. And accordingly, having moiſten'd the ſurfaces with a due Proportion of good expreſs'd Oyl of ſweet Almonds, and having carefully obſerv'd the other requiſite Circumſtances, we took up ſome drams above four hundred Ounces Troy hanging at the lower Marble.

And

And that you may not ſuſpect that it was by glewing the Marbles together, that the Oyl did enable them to make ſo much greater reſiſtance againſt ſeparation than the Spirit of Wine did; I ſhall adde, That in caſe the flat ſurfaces of the joyn'd Stones were held not parallel, but perpendicular to the Horizon, that ſo the air might (as we formerly alſo obſerv'd) immediately ſucceed as the looſer Marble ſhould ſlide off, the weight of ſome Ounces was now and then requiſite to draw down the Marbles when they had nothing but Spirit of Wine between them, whereas they would eaſily enough ſlide off from one another when they were cemented together with Oyl: perhaps becauſe that the Spirit of Wine by reaſon of the ſmallneſs and penetrancy of its parts, and becauſe of its fugitive nature, did not ſo well fill up the little pores and furrows of the ſurfaces of the Marbles; whereby the little protuberances getting into thoſe little cavities, might more reſiſt the ſliding of the Marbles upon one anothers ſurfaces, whoſe texture is better fitted to make their ſurfaces ſmooth and ſlippery.

And to ſhew that the reſiſtance of ſuch contiguous Marbles to a violent ſeparation is greater in thoſe which being broader are preſs'd againſt or reſiſted by a proportionably bigger (though not a longer) Pillar of the Atmoſphere; We cauſed two Marbles to be made, whoſe Diameter was three inches or a very little more: and having after the abovemention'd manner employ'd Spirit of Wine to keep out the air from between them, we did after ſome tryals, with the uppermoſt of them take up the lowermoſt, and with it four hundred ſixty eight, or four hundred and ſeventy Ounces. But making uſe of Oyl of Almonds inſtead of Spirit of Wine, we did with our own hands draw up twice, one time after another, with the undermoſt Stone, a much greater weight, namely, eighty four pound or 1344 Ounces (Troy weight) notwithſtanding which

weight

weight we manifeſtly perceiv'd the Marble at which it hung to ſtick ſtrongly to the other.

And here again we will take notice, that the interpos'd Oyl was ſo far from being able as a Cement, rather than by keeping out the air, to make theſe Marbles ſtick ſo cloſe together, that, whereas Bodies glew'd or cemented together are wont to make an almoſt equal reſiſtance to their being ſeparated, in what Poſture ſoever you place them, I made our Marbles, even when we try'd this laſt Experiment, very freely ſlide upon one another, by impelling the upppermoſt to the right hand or to the left, with my Finger or my Thumb: (whereof the reaſon is intimated above, where we mention'd almoſt the like caſe in Glaſſes) and having ſometime before taken up a weight which we conjectur'd to be not much inferiour to that laſt named, we preſently for tryal ſake held the Marbles with their edges downwards, and found that thoſe that in an Horizontal Poſition could not be drawn aſunder by ſo great a weight, did in another poſture preſently fall aſunder by their own weight, which made one readily ſlide off from the other to the ground. Now although we have confeſs'd, that this way of meaſuring the force of the Air is not Accurate; yet we hope it will not be thought Uſeleſs, ſince (not to mention that by thus breaking the Ice, we may make way for the happier Enquiries of others) it not only ſhows us that this preſſing or ſuſtaining force of the air, as unheeded as it is wont to be, is very *Great*, but it may alſo aſſiſt us to conjecture *how Great* it is, which though we cannot hereby determine preciſely and with certainty, yet we may eſtimate it with much leſs uncertainty than otherwiſe we could.

I know that the Peripateticks, and the generality of the School Philoſophers, will confidently aſcribe the ſticking of the Marbles, not to the cauſe we have aſſign'd, but to Natures Abhorrency and fear of a Vacuum. But not

to

to engage our ſelves now in a Diſquiſition that when we diſcours'd of Fluidity we did (for the Reaſons there expreſs'd) decline to meddle with; We will, without diſputing whether or no Nature either can at all admit, or do abhorre a Vacuum, content our ſelves to confirm the Explication given of this Phænomenon by theſe two Conſiderations. The one, That if Nature did ſo violently oppoſe a Vacuum as is pretended, it is not likely that any force whatſoever that we could employ would be capable to produce one; whereas in our caſe we find, that a little more weight added to the lower of the Marbles, is able to ſurmount their Reluctancy to ſeparation, notwithſtanding the ſuppos'd danger of thereby introducing a Vacuum. And my next Conſideration is, that (according to what we have hitherto deliver'd) without having recourſe to any ſuch diſputable Principle, a fair account may be given of the propos'd Phænomenon, by the preſſure or weight of the Air. And that what we have ſaid concerning the latter of theſe may be entertain'd with the leſs difficulty, let us ſuppoſe, that when the Marbles ſtick well together, the lowermoſt of them, or the appendant weight were faſten'd to the ground: For in this caſe there appears no reaſon to believe that their power to reſiſt ſeparation would be leſs than it was before. And yet it ſeems evident, that the uppermoſt Marble would not be perpendicularly pull'd up but by ſuch a force as were at leaſt (I ſay at leaſt) able to lift up a weight equal to that of the laſt mention'd Marble, and of a Pillar of Air having the Stone for its Baſe, and reaching to the top of the Atmoſphere; ſince at the inſtant of Revulſion, before the Air can get in, and ſpread it ſelf between the Stones, there is not for ought appears any ſuch Body under the upper Marble, as can help the hand to ſuſtain the weight both of that Marble and the incumbent Cylinder of the Atmoſphere, which then gravitates upon it, and conſequently

quently upon the hand; because there is no Air, nor other equivalent Body underneath it, to sustain its part of the weight, as the lower Air is wont to do in reference to the heavy Bodies that lean on it, and to the weight of the incumbent Air. And therefore we need not much marvel, if when only a less weight than that of the foremention'd Pillar of the Atmosphere hangs at the lower Marble, it should be capable of being drawn up by the uppermost, rather than suffer a divulsion from it. As we see that when two Bodies being fasten'd together, are endeavour'd to be drawn asunder by forces or weights not able to separate them, they will usually both of them move that way, towards which either of them is the most strongly drawn. On which occasion, I remember what I have sometimes observ'd in one of the wayes of trying the strength of Load-stones: For if the Load-stone be able to take up more than its own weight, you may as well lift up the Load-stone by a Knife, as the Knife by the Load-stone. And though one accustom'd to judge only by his Eyes, would have imagin'd, that when I held the great weights formerly mention'd suspended in the Air, there was no strong endeavour to pull up the upper Marble from the lower, because my hand being for a while held steddy, seem'd to be at rest; yet he will easily be invited to suspect that in such a thought there may be a great mistake, who shall consider, that neither did the weight sensibly appear to pull the lower Marble downwards, though my hand assur'd me that the weight had not lost its Gravitation. And if I shall adde, that once, when the weight after having been lifted up into the Air, was casually so loosen'd from the upper Marble, as suddenly to drop down, my hand, unawares to me, was by the force of that Endeavour it just before employ'd to sustain the fallen weight, carried up with such violence, that I very sensibly bruis'd it by the stroak it gave against the face of a By-

ſtander, who chanc'd out of curioſity to hold his Head over the Marbles.

And here it will not be impertinent to bring in an Experiment that I once devis'd, not only for other uſes, but to illuſtrate the ſubject we have been hitherto treating of. The Tryal I lately found regiſtred among my *Adverſaria*, in theſe Termes. A Braſs Valve of about an Inch Diameter was with Cement well faſtned to the ſhorter Leg (which was but of very few Inches in Length) of a long Glaſs Syphon left open at the end of the other Leg. This Valve being let down to the Bottom of a tall Glaſs Body full of water, ſo that 'twas (if I much miſ-remember not) between a Foot and half a yard beneath the ſurface of the water, when there was let in as much water into the Pipe, as reach'd in that as high as the ſurface of the External Water in the Tall Cucurbite: Then about an Ounce weight was put into the oppoſite Scale of a Ballance, to the neighbouring Scale whereof one end of a ſtring was tyed, whoſe other end was faſtned to the ſaid Valve, whoſe parts would be thereby drawn aſunder. But when the water was empty'd out of the Pipe, and the Valve was let down to the former depth, there was requiſite about 5 Ounces, that is 4 Ounces more than formerly, to disjoin the parts of the Valve, and let the water get in between: And when (the Syphon being freed from water) the Valve was lifted higher and higher, together with the Pipe, there needed leſs and leſs weight to make a Disjunction; two Ounces of Additional weight (to the one Ounce requiſite to counterpoize the Cover of the Valve it ſelf) on the water ſufficing to lift up the Cover, when the Valve was held about half way, between its Lower ſtation, and the Top of the water; a ſingle Ounce ſufficing afterwards, and half an Ounce of Additional weight proving enough to disjoin the parts when the Valve was held but a little beneath the ſurface of the Liquor.

This

This relation of an Experiment which I afterwards ſhow'd to many *Virtuoſi*, will perhaps ſeem ſomewhat dark to you without a Scheme; but if you conſider it attentively enough to apprehend it throughly, I preſume it will ſhow you, that whether or no there be upon any other ſcore a repugnancy to the ſeparation of ſmooth Bodies join'd by immediate contact; yet certainly there may be a great Repugnancy upon the bare Account of the Gravity of the medium, wherein the Divulſion is attempted. For in our caſe the *Fuga Vacui*, if there be any, ought to reſiſt the ſeparation of the Parts of a Valve ſtill kept under water, as much near the Top of the water, as at the Bottom. And therefore the great difference found in that reſiſtance at thoſe different places, may be attributed to the Preſſure of the Ambient water, that thruſt them together. And though it be true that Air is an Exceeding Light Body in compariſon of water; yet in divers Tryals I have found the Diſproportion in Gravity of thoſe two Fluids not to exceed that of a 1000. to 1. So that conſidering how many miles, not to ſay ſcores of miles, the Air may reach upwards, there ſeems no abſurdity at all to ſuppoſe that the bare Preſſure of it againſt the Marbles formerly mention'd, may keep them as coherent as we found them to be.

But ſince this I have been able to make an Experiment, that does ſufficiently confirm the former Doctrine. For having ſuſpended the two coherent Marbles in a Capacious Glaſs, whence by a certain contrivance, the Air could little by little be drawn out, we found as we expected, that whilſt there remain'd any conſiderable quantity of Air in the Glaſs, the lower Marble continued to ſtick to the other, the Preſſure of the remaining Air, though but weak, being yet ſufficient for the ſuſtentation of the lower Marble, which it was not after the Air was further withdrawn. And if when the Disjunction was made, the upper Marble were by another contrivance let down upon the lower, ſo as to touch it as before; though whilſt the External Air was kept out of the Glaſs, the upper Marble might eaſily be rais'd without taking up the lower with it; yet when the outward Air was let in, the Marbles were preſs'd together, and became again ſtrongly coherent.

But it is now high time to look back to that part of our Diſcourſe, which the conſideration of our Marbles has ſo long intic'd us from directly proſecuting. Although then it may from the paſt Diſcourſe be conceiv'd, that in Bodies of ſenſible bulk, whoſe ſmooth ſurfaces touch one another, the force of the Air does mainly make them cohere; yet it ſeems that generally in Bodies (whether greater or ſmaller) it is a ſufficient cauſe of Coheſion that the parts of the Body are at reſt by one another, though perhaps the entire Concretion be remov'd from place to place. For Bodies of ſenſible bulk being either fluid or conſiſtent, and it being (as above we have taught) the chief requiſite of a fluid Body that its ſmall parts be in motion, there ſeems not any thing neceſſary to keep a Body from being fluid, and conſequently to keep it a firm Body, but that its contiguous parts be in a ſtate of reſt.

I know that almoſt all Philoſophers both ancient and modern, require ſomething elſe than the Reſt of the parts (of which ſcarce any of them takes the leaſt notice as of a thing conducive to Firmneſs) to the keeping together the parts of a dry and ſtable Body. But although to engage very far in ſuch a Metaphyſical and nice Speculation were unfit for me, (at leaſt at preſent, when I am but to endeavour to explicate Fluidity and Firmneſs in the ſenſible Bodies we converſe with;) yet we dare not quite ſkip it over, leſt we be accus'd of Overſeeing it. The greater number of Contemplators aſcribe the effect under Conſideration to a certain ſubſtantial form, to which they aſſign, among other Offices, that of keeping all the parts united into one Body. But what this form is, and by what means it unites the parts ſo ſtrongly in a Diamond or a Ruby, &c. and ſo looſely in Tallow, Camphire, or the like ſlight concretions, and how the ſubſtantial form continuing the ſame in Water and Ice, the ſame matter may eaſily and frequently become by turns a hard and a fluid

Body;

Body; how theſe, I ſay, and diverſe others things are effected by the forms of ſolid Bodies, is to me, I confeſs, at leaſt as difficult to conceive, as to imagine without it a cauſe of Coheſion in the parts of a dry Body.

Other Learned Men there are among the modern Naturaliſts who have recourſe ſome of them to a Spirit, which penetrating and faſtening to each other all ſubſtances corporeal, unites them into one World; but others fancy rather a certain Cement or Glue, whereby they conceive the parts of Bodies to be made as it were to ſtick to each other. But as for this laſt Hypotheſis, it would be conſider'd, that though Glue is made uſe of to joyn together Bodies of ſenſible bulk, yet Glue it ſelf being a Body that is ſo too, it muſt alſo it ſelf conſiſt of leſſer parts ſticking to one another; which allows me to demand the cauſe of the mutual Coherence of thoſe parts. And if it be anſwer'd, that they likewiſe ſtick together by the Intervention of ſome more ſubtil Glue, I ſhall again repreſent that this Glue alſo muſt conſiſt of corporeal parts; and therefore I ſhall further demand how theſe alſo ſtick together: and if the like Anſwer be again made me, I ſhall ſtill renew the like Demand, till at length the Anſwerer be reduc'd to confeſs, that parts of Matter ſo very ſmall cannot be reaſonably ſuppos'd to be kept together by a Cement. And if the Corpuſcles that make up the fineſt Glue imaginable are not kept together by a Cement, we may be allow'd to aſcribe their Adheſion to the immediate Contact and Reſt of the component parts, (which is a cauſe intelligible, and at leaſt probable) till ſome other ſufficient cauſe be aſſign'd, which I do not take that to be which is taught by the Patrons of the firſt Hypotheſis lately mention'd, concerning a Spirit diffus'd through the maſs of matter. For not to mention that the Agility of a Spirit ſeems not ſo proper a Qualification for that which is to faſten Bodies together, we may conſider, that this ſub-

ſtance

ſtance which is called a Spirit is indeed but a ſubtil Body. And why therefore may not the minute parts of other Bodies, if they be conveniently ſhap'd for Adheſion, ſtick to one another, as well as ſtick to this Spirit? And I ſhould here alſo demand, how the parts of this Spirit are kept faſt to one another. If any ſhould anſwer, That this Spirit conſiſts of parts which are inſeparable, and yet perhaps of a hooked ſhape, which fits them to faſten themſelves to the Bodies they take hold of, and thereby thoſe to one another; this would be to propoſe ſuch a new Notion of the diffus'd Spirit, as I know not whether thoſe whoſe Opinion I have been examining did ever dream of, or would be content to adopt: and ſure according to this Hypotheſis there muſt be a wonderful plenty of theſe little Spirits in the groſſeſt Bodies; and Ice for example, which is thought ſo deſtitute of Spirits, muſt be well-nigh half made up of them: For theſe little ſpirituous parts can faſten no parts of other Bodies together but thoſe they touch, ſince otherwiſe the parts of other ſorts of matter, if but contiguous, might cohere without theſe, which is againſt the Hypotheſis. And ſince each of theſe ſmall ſpirituous Corpuſcles, if I may ſo call them, being really a natural Body, and by conſequent neceſſarily diviſible, at leaſt by Thought, into parts, I ſhall aſk the Propoſers of this new Notion of Spirit, upon what Account this Corpuſcle can be indiviſible; I mean, what it is, if it be not Reſt and immediate Contact, that hinders but that the parts (or deſignable parcel of corporeal ſubſtance) which are diviſible by thought, ſhould be alwayes kept together, and never be actually divided. I am not averſe indeed from granting, that they may almoſt alwayes eſcape Diſſolution: but I am apt to ſuſpect that may be, becauſe that by reaſon of the extreme ſmallneſs, and the Reſt and ſtrict Contact of their parts, they can ſcarce ever meet with an Agent minute and ſwiftly enough mov'd,

to

to be able to ſhatter them or diſſociate the combin'd parts. For to ſay, that 'tis the Nature of every ſuch Corpuſcle to be indiviſible, is but to give me cauſe to demand how that appears: For ſo important an Aſſertion needs more than a bare Affirmation for proof.

And if two of theſe Corpuſcles that are preſum'd to be indiviſible ſhould, being ſmooth and of the ſame figure, (as for Example Cubical) happen to lye upon one another, and a third ſhould likewiſe chance to be fitly plac'd upon the uppermoſt of the two, what ſhould hinder but that this Aggregate may by the violent knock of ſome other Corpuſcles be broken in the midſt of the whole Concretion, and conſequently in the middlemoſt body? For ſuppoſe them as Adamantine as you pleaſe, yet ſince Corpuſcles as hard as they are, can be made very violently to knock againſt them; why may not theſe grate or break the middlemoſt Corpuſcles, or any of the others? as we ſee that Diamonds themſelves may be reduc'd to powder by other Diamonds, though not (as Artificers vers'd in the Trade inform me) by Attrition with any other ſtone. To prove that the Coheſion of the middlemoſt of the three lately-mention'd ſmall Deyes with the other two, the one above it, the other below, is not ſo ſtrong as that of the parts of that middlemoſt Corpuſcle, notwithſtanding that the contact between each two adjoyning Bodies is ſuppos'd to be full, (for ſo it muſt be in ſuch Bodies, though not alwayes in others viſibly greater, in which ſome ſubtiler ſubſtance may be ſuppos'd to come in part between them;) to prove this, I ſay, there muſt be aſſign'd ſome better cauſe of the Coheſion of the matter in one part of the propos'd Body than in the other. And it cannot with probability be pretended, that a Corpuſcle preſum'd uncapable to be divided ſhould conſiſt of hooked parts: and if that ſhould be pretended, yet ev'n theſe hooks alſo being true Bodies, the Queſtion would recur concerning

Them,

ſtance which is called a Spirit is indeed but a ſubtil Body. And why therefore may not the minute parts of other Bodies, if they be conveniently ſhap'd for Adheſion, ſtick to one another, as well as ſtick to this Spirit? And I ſhould here alſo demand, how the parts of this Spirit are kept faſt to one another. If any ſhould anſwer, That this Spirit conſiſts of parts which are inſeparable, and yet perhaps of a hooked ſhape, which fits them to faſten themſelves to the Bodies they take hold of, and thereby thoſe to one another; this would be to propoſe ſuch a new Notion of the diffus'd Spirit, as I know not whether thoſe whoſe Opinion I have been examining did ever dream of, or would be content to adopt: and ſure according to this Hypotheſis there muſt be a wonderful plenty of theſe little Spirits in the groſſeſt Bodies; and Ice for example, which is thought ſo deſtitute of Spirits, muſt be well-nigh half made up of them: For theſe little ſpirituous parts can faſten no parts of other Bodies together but thoſe they touch, ſince otherwiſe the parts of other ſorts of matter, if but contiguous, might cohere without theſe, which is againſt the Hypotheſis. And ſince each of theſe ſmall ſpirituous Corpuſcles, if I may ſo call them, being really a natural Body, and by conſequent neceſſarily diviſible, at leaſt by Thought, into parts, I ſhall aſk the Propoſers of this new Notion of Spirit, upon what Account this Corpuſcle can be indiviſible; I mean, what it is, if it be not Reſt and immediate Contact, that hinders but that the parts (or deſignable parcel of corporeal ſubſtance) which are diviſible by thought, ſhould be alwayes kept together, and never be actually divided. I am not averſe indeed from granting, that they may almoſt alwayes eſcape Diſſolution: but I am apt to ſuſpect that may be, becauſe that by reaſon of the extreme ſmallneſs, and the Reſt and ſtrict Contact of their parts, they can ſcarce ever meet with an Agent minute and ſwiftly enough mov'd,

to

to be able to ſhatter them or diſſociate the combin'd parts. For to ſay, that 'tis the Nature of every ſuch Corpuſcle to be indiviſible, is but to give me cauſe to demand how that appears: For ſo important an Aſſertion needs more than a bare Affirmation for proof.

And if two of theſe Corpuſcles that are preſum'd to be indiviſible ſhould, being ſmooth and of the ſame figure, (as for Example Cubical) happen to lye upon one another, and a third ſhould likewiſe chance to be fitly plac'd upon the uppermoſt of the two, what ſhould hinder but that this Aggregate may by the violent knock of ſome other Corpuſcles be broken in the midſt of the whole Concretion, and conſequently in the middlemoſt body? For ſuppoſe them as Adamantine as you pleaſe, yet ſince Corpuſcles as hard as they are, can be made very violently to knock againſt them; why may not theſe grate or break the middlemoſt Corpuſcles, or any of the others? as we ſee that Diamonds themſelves may be reduc'd to powder by other Diamonds, though not (as Artificers vers'd in the Trade inform me) by Attrition with any other ſtone. To prove that the Coheſion of the middlemoſt of the three lately-mention'd ſmall Deyes with the other two, the one above it, the other below, is not ſo ſtrong as that of the parts of that middlemoſt Corpuſcle, notwithſtanding that the contact between each two adjoyning Bodies is ſuppos'd to be full, (for ſo it muſt be in ſuch Bodies, though not alwayes in others viſibly greater, in which ſome ſubtiler ſubſtance may be ſuppos'd to come in part between them;) to prove this, I ſay, there muſt be aſſign'd ſome better cauſe of the Coheſion of the matter in one part of the propos'd Body than in the other. And it cannot with probability be pretended, that a Corpuſcle preſum'd uncapable to be divided ſhould conſiſt of hooked parts: and if that ſhould be pretended, yet ev'n theſe hooks alſo being true Bodies, the Queſtion would recur concerning

Them,

Them, and be ſtill renewable *in infinitum.* If it ſhould be ſaid that theſe minute Bodies are indiſſoluble, becauſe it is their nature to be ſo; that would not be to render a Reaſon of the thing propos'd, but in effect to decline rendring any. And though I know that in every Hypotheſis about the principles of things, ſomething is allow'd to be aſſum'd, as not being to be explain'd or prov'd by any thing more primary than it ſelf; yet I know not whether this excuſe be proper in our Caſe, wherein it ſeems that the entireneſs and permanency of any parcel of matter, how minute ſoever, may be probably enough deduc'd from the immediate Contact, the Reſt and the extreme littleneſs of its deſignable parts. And if for a laſt refuge it ſhould be ſaid, that the deſignable parts of theſe Corpuſcles are therefore unſeparable, becauſe there is no vacuity at all intercepted between them; beſides that this is contrary to the ſuppoſition formerly made; for ſuch extremely minute Deyes as we imagin'd to be one upon another, having their ſurfaces according to our Poſtulatum, flat, ſmooth, and exquiſitely congruous, could no more than the parts of either of the three Corpuſcles have any vacuity intercepted between them: beſides this, I ſay, this is both to ſuppoſe a Vacuum in all diviſible Bodies, and that too as the cauſe of their being ſuch, and to decline the former Hypotheſis touching the uſe of this Spirit, and take Sanctuary among the Atomiſts, to whoſe opinion about the account upon which thoſe Bodies they call Atoms are not diſſipated, although ſome of the Conſiderations we have alledg'd againſt the newly examin'd opinion may in part be appli'd; yet diverſe of their other opinions do ſo fairly comport with the generality of our Experiments in theſe Notes touching Fluidity and Firmneſs, that I am willing to decline claſhing with them, by not purſuing now any further a Diſquiſition, which, as I ſaid a while ago, is not neceſſary to my preſent deſign: eſpe-

cially, ſince the dim and bounded Intellect of man ſeldom proſperouſly adventures to be Dogmatical about things that approach to Infinite, whether in vaſtneſs or littleneſs. Nor indeed would I have that look'd upon as a reſolute Declaration of what I think of ſo abſtruſe a Subject, which I have rather propos'd to avoid ſaying nothing, where I ſuppos'd it expected I ſhould ſay ſomething.

The other thing then, which in our Deſcription of a firm Body we mention'd as capable to make it ſo, is the texture of the parts (whether homogeneous or not) that conſtitute it; and though the Juxta-poſition and Reſt of theſe parts may poſſibly alone ſuffice to make the Body ſtable; yet this Texture ſeems to be the moſt uſual cauſe of ſtability, and ſometimes alſo it may ſuperadde a degree of that quality to that which bodies may have upon the former account only. For, though whilſt the parts of the Body are actually at reſt it cannot be fluid, yet thoſe parts, if they cohere to one another but by reſt only, may *Cæteris paribus* be much more eaſily diſſociated and put into motion by any external Body actually mov'd, than they could be if they were by little hooks and eyes, or other kind of faſtenings intangl'd in one another; it being often neceſſary in this caſe violently to break off theſe faſtenings before the little bodies faſten'd together by them can be disjoyn'd, and put into ſuch a ſeparate motion as is requiſite to the conſtituting of a fluid Body.

We formerly made uſe of that familiar ſubſtance, the white of an Egge, to illuſtrate the nature of Fluidity: Let us now try whether it will alſo aſſiſt us in our enquiry after the cauſes of ſtability. When an Egge is made hard by boyling, ſince whether we ſuppoſe this Induration to be effected by bare motion or impulſe, or elſe by the inſinuation of fiery Corpuſcles, ſince I ſay there is nothing that appears to get in at the ſhell, unleſs perhaps ſome calorifick Atoms, and perchance too ſome little particles of the fluid water it is

Them, and be ſtill renewable *in infinitum*. If it ſhould be ſaid that theſe minute Bodies are indiſſoluble, becauſe it is their nature to be ſo; that would not be to render a Reaſon of the thing propos'd, but in effect to decline rendring any. And though I know that in every Hypotheſis about the principles of things, ſomething is allow'd to be aſſum'd, as not being to be explain'd or prov'd by any thing more primary than it ſelf; yet I know not whether this excuſe be proper in our Caſe, wherein it ſeems that the entireneſs and permanency of any parcel of matter, how minute ſoever, may be probably enough deduc'd from the immediate Contact, the Reſt and the extreme littleneſs of its deſignable parts. And if for a laſt refuge it ſhould be ſaid, that the deſignable parts of theſe Corpuſcles are therefore unſeparable, becauſe there is no vacuity at all intercepted between them; beſides that this is contrary to the ſuppoſition formerly made; for ſuch extremely minute Deyes as we imagin'd to be one upon another, having their ſurfaces according to our Poſtulatum, flat, ſmooth, and exquiſitely congruous, could no more than the parts of either of the three Corpuſcles have any vacuity intercepted between them: beſides this, I ſay, this is both to ſuppoſe a Vacuum in all diviſible Bodies, and that too as the cauſe of their being ſuch, and to decline the former Hypotheſis touching the uſe of this Spirit, and take Sanctuary among the Atomiſts, to whoſe opinion about the account upon which thoſe Bodies they call Atoms are not diſſipated, although ſome of the Conſiderations we have alledg'd againſt the newly examin'd opinion may in part be appli'd; yet diverſe of their other opinions do ſo fairly comport with the generality of our Experiments in theſe Notes touching Fluidity and Firmneſs, that I am willing to decline claſhing with them, by not purſuing now any further a Diſquiſition, which, as I ſaid a while ago, is not neceſſary to my preſent deſign:

eſpe-

cially, ſince the dim and bounded Intellect of man ſeldom proſperouſly adventures to be Dogmatical about things that approach to Infinite, whether in vaſtneſs or littleneſs. Nor indeed would I have that look'd upon as a reſolute Declaration of what I think of ſo abſtruſe a Subject, which I have rather propos'd to avoid ſaying nothing, where I ſuppos'd it expected I ſhould ſay ſomething.

The other thing then, which in our Deſcription of a firm Body we mention'd as capable to make it ſo, is the texture of the parts (whether homogeneous or not) that conſtitute it; and though the Juxta-poſition and Reſt of theſe parts may poſſibly alone ſuffice to make the Body ſtable; yet this Texture ſeems to be the moſt uſual cauſe of ſtability, and ſometimes alſo it may ſuperadde a degree of that quality to that which bodies may have upon the former account only. For, though whilſt the parts of the Body are actually at reſt it cannot be fluid, yet thoſe parts, if they cohere to one another but by reſt only, may *Cæteris paribus* be much more eaſily diſſociated and put into motion by any external Body actually mov'd, than they could be if they were by little hooks and eyes, or other kind of faſtenings intangl'd in one another; it being often neceſſary in this caſe violently to break off theſe faſtenings before the little bodies faſten'd together by them can be disjoyn'd, and put into ſuch a ſeparate motion as is requiſite to the conſtituting of a fluid Body.

We formerly made uſe of that familiar ſubſtance, the white of an Egge, to illuſtrate the nature of Fluidity: Let us now try whether it will alſo aſſiſt us in our enquiry after the cauſes of ſtability. When an Egge is made hard by boyling, ſince whether we ſuppoſe this Induration to be effected by bare motion or impulſe, or elſe by the inſinuation of fiery Corpuſcles, ſince I ſay there is nothing that appears to get in at the ſhell, unleſs perhaps ſome calorifick Atoms, and perchance too ſome little particles of the fluid water it is

boyled in, 'tis not eaſie to diſcover from whence elſe this change of conſiſtency proceeds, than from a change made in the texture of the parts whereby they are connected and diſpos'd after a new manner, fit to make them reciprocally hinder the freedom of each others motions. But if inſtead of hardning the whites of Eggs by the heat of the fire, you beat them very well into froth, you may perceive that froth to emulate the nature of a ſtable Body: for not only you may raiſe it up to a pretty height, and make it retain a ſharp top almoſt like a Pyramide; but I remember I have for curioſity ſake made with a little care a long and proportionably thick Body of theſe bubbles hang down from my finger without falling, like an ice-icle from one of the Reeds of a thatch'd Houſe, and yet in this there appears not any alteration to be made in the fluid Body, ſave a meer Mechanical change of the diſpoſition of its parts: which may be confirmed by water beaten into froth, for there the heaped bubbles will quickly ſubſide and fall back into water of the very ſame conſiſtence it was of before.

Now there may be ſeveral things whereby a body may be put into ſuch a texture as is convenient to make it firm or ſtable. And of theſe, before we conſider of them particularly, it will be fit to take notice in general, that for the moſt part 'tis not from any of them Single, but from two or more of them Concurring, that the Stability of Concretions proceeds.

The firſt and chiefeſt of theſe ſeems to be the fitneſs of the ſhapes of the component particles to faſten to each other; as if ſome were figur'd like the handles of Buckets, and others like the hooks that are wont to be employ'd to draw them up out of the Well, or ſome like buttons, others like loops, ſome like male, others like female ſcrews (as Mechanicians ſpeak) or as if many together were ſo variouſly branch'd, that their parts may be ſo interwoven one within another, as not to be eaſily ſeparable (as we often ſee in

a

a well-made dry hedge, of which if a man go to pull away one bough, he ſhall often be unable to do it without pulling away with it diverſe others whoſe ſlender twigs will be intangled with it.) An eminent example of the power of the bare Texture of many ſmall Bodies (even ſuch as each of them apart is not perhaps extraordinarily ſhap'd for ſuch a purpoſe) to make a ſtable one, is afforded us by Ropes and Cables; where only by twiſting together and wreathing the ſlender and flexible threds the Cable is made up of, they are ſo well as it were wedg'd in between and faſten'd to one another, that they conſtitute a Body not to be broken by the weight of an Iron Anchor, nor perhaps by the force of a Ship violently driven on by the fury of the Winds and Waves. This figuration of the Corpuſcles that make up conſiſtent Bodies, ſeems to have been the chief if not only cauſe of their conſiſtence in the Judgment of the antient Atomiſts, this being the account that is given of it by *Lucretius*.

Denique quæ nobis durata ac ſpiſſa videntur,
Hæc magis hamatis inter ſeſe eſſe neceſſ' eſt,
Et quaſi ramoſis alte compacta teneri.
In quo jam genere imprimis adamantina ſaxa,
Prima acie conſtant, ictus contemnere ſueta,
Et validi ſalices, ac duri robora ferri,
Æraque quæ clauſtris reſtantia vociferantur.

And indeed, ſo innumerable may be the correſpondent figures which are fit to faſten bodies to one another, that it is very poſſible that two bodies, whereof each a part is fluid, may upon their Conjunction immediately intangle their parts in one another, and thereupon acquire ſuch a new texture, that their parts cannot as formerly diſſociate themſelves at pleaſure, and move along one anothers ſurfaces, nor conſequently flow after the manner of Liquors, but are

ſo connected or intangled, that the motion of one of them will be reſiſted by many, and ſo the whole Body will become firm or ſtable. Something like this may be ſeen in the Experiment mention'd by our Author, where he teaches that the diſtill'd Liquor of Nitre, and that made *per Deliquium* out of fix'd Nitre, will preſently upon their mixture in part concoagulate into ſaline and conſequently ſtable Bodies: but this ſeeming only a re-union of the ſaline particles that did, though inviſibly, ſwim up and down in the aqueous parts of the mingled Liquor, which after this ſeparation remains both more copious than the ſaline parts, and as fluid as before, we will adde a noble inſtance (mention'd to another Purpoſe by *Lully* and *Hartman*) to declare how much the firmneſs of bodies depends upon their texture. If you take then the Alchool or highly-rectify'd Spirit of Wine, and exquiſitely deflegm'd Spirit of Urine, and mix them in a due proportion (as I remember the laſt time I made the Experiment I took about equal parts by gueſs, though two of the former to but one of the latter, if This be excellent, be a better Proportion;) you may in and about a minute of an hour turn theſe two fluid Liquors into a conſtant Body; and I confeſs it was not without pleaſure, that I have immediately upon the ſhaking of theſe two Liquors ſeen them ſhoot into the likeneſs of Snow, and acquire ſuch a conſiſtence, that I could without ſpilling the mixture turn the veſſel that contain'd it upſide down.

But I dare not expect to have this Experiment believ'd ev'n by moſt of them that ſhall try it, Experience having taught me, that it will not ſucceed, unleſs the Spirits of Urine and of Wine be both of them more exactly deflegm'd than is uſual even among Chymiſts. Yet ſo much more does this coagulation ſeem to depend upon the Salt of Urine as of ſuch a texture, than barely as Urinous, that we will add that, As the ſpirit of fermented Urine is not (whatſoever ſome eminent Chymiſts may think or ſay) ſo indiſpenſably requiſite,

quiſite, but that my curioſity leading me to try whether other Liquors, which I ſuppos'd to be of a reſembling nature, might not ſerve the turn, I found that ſufficiently-rectifyed Spirit of Harts-horn (to mention that alone here) may be made to ſupply its place: So I endeavour'd to make it probable by this, That having try'd a certain method (though that may ſeem ſtrange to moſt Chymiſts) of ſo ordering Urine, that without ſtaying at all to ferment or putrifie it either forty days or half ſo many hours, I can make the volatile or ſaline Spirit aſcend firſt in diſtillation though I uſe but ſome ſuch gentle heat as that of a Bath: Having, I ſay, by this means diſtill'd a very ſtrong Spirit of unfermented Urine, and rectify'd it too, I found, as I expected, that I could not by any means make it coagulate with Spirit of Wine, which ſeem'd to proceed from the differing texture of this Spirit from that of Fermented, or rather Putrify'd, Urine, ſince I had added nothing to the freſh Urine I diſtill'd, but what was extreamly fix'd and belonging (as Chymiſts ſpeak) to the mineral Kingdom. Whereupon having had the curioſity to enquire of ſome of my Chymical acquaintances, I found that they complain'd that they had not been able to coagulate Spirit of Wine with the ſaline Spirit made of meer Urine, without any addition at all, when they diſtill'd that Urine without a previous putrefaction (which is not wont to be perfected under ſix weeks or thereabouts.) But to return to our Coagulum we will annex, That this is further remarkable in this Experiment, that this white coagulated ſubſtance being put into a glaſs veſſel exactly ſtopt and kept in a gentle heat (which yet it ſelf is not perhaps neceſſary, though expedient) for ſome weeks or months, will at leaſt for the greateſt part by much (for I have not yet totally ſeen it do ſo) reſume the form of a limpid Liquor; as if either all the crooked particles that connected the ſmall coalitions of the Vinous and Urinous Cor-

This Way the Author afterwards Publiſhed in another Book.

Corpuſcles to one another, were by the motion they were put into by the external heat one after another broken off, or elſe the ſame little concretions (for the *Menſtruum* ſeems to conſiſt chiefly of them, being able to perform other matters than either of the ſingle Liquors whereof 'tis conſtituted) either aſſiſted by outward warmth, or inabled thereto by ſome other cauſe of mobility, did after many and various attempts to clear themſelves of each other, little by little ſo unbend or break off the crooked particles that intangled them, as at length to extricate themſelves, and become capable of freely ſhifting places among themſelves, and ſo of the form of a Liquor.

And here I ſhall adde a couple of Experiments for the ſake of their affinity with ſome parts of the newly recited Experiments about the Vino-Urinous Coagulate.

And firſt it ſeem'd to me worth trying, whether ſome Acid Salts being duly order'd would not concoagulate with Spirit of Wine, as well as with Urinous Salts; and having for a while digeſted together in a convenient proportion pure Saccharum Saturni, made with Spirit of Vinegar, and rectify'd Spirit of Wine, I found the mixtures ſo chang'd in point of Conſiſtence, that upon inclining the Glaſs which contained it, none of it would run down the Sides. But this Experiment did not afterwards ſeem to me either eaſie or conſiſtent, nor is it more then one of the ways, and I doubt none of the beſt, of attempting what we have propos'd.

The other Experiment I promis'd you, relates to the reſolution of the Coagulum of Spirit of Wine (which to be performed by digeſtion requires a very long time) And I wiſh I had not ſome reaſons to hinder me from communicating to you the way of making of it at preſent; it being an Experiment that ſeems ſomewhat ſtrange in its kind; but that part of it which is directly pertinent to our preſent Argument, you will, I truſt, believe upon my Relation, which

which is, that by the addition of nothing but of a very fixt and very dry Body (insomuch that 'twill not yield any thing by the common way of distillation even in a naked fire) by the sole addition, I say, of this dry Body, the newly mention'd Coagulum, which is also a consistent Body, may in a few hours be brought into a permanent Liquor (quite distinct from the dry Body) which when Experience first recommended this way to me, was of too subtle and penetrating a Nature, not to make me expect from it considerable Effects, both in Chymistry and Physick, of which imitation you may be pleas'd to take notice.

But to return to what I was about to subjoyn after the mention of our Coagulum, as that is an Example of firmness produced by Texture; I will here, because it is not easie to procure Spirits pure enough to make such a Concretion as that. I will here, I say, set down another way of speedily hardning one fluid Body by another; for if you take the white of an Egge, and beat it till it become thin, and then shake well into it about half its quantity (perhaps much less might serve the turn) of right Spirit of Salt, you shall have in a few minutes the mixture so coagulated, that I remember when we turn'd the glass wherein we made it upside down, not a drop of Liquor did run out, though some hours after we obtain'd a little by breaking the crudled matter. And another Experiment much of the nature of this is said to be delivered by Sir *Francis Bacon*, who teaches to coagulate whites of Eggs with Spirit of Wine: and indeed, if you observe a circumstance (unmention'd, that I hear of, by him) which is the shaking of the two Bodies well together, and if your Spirit of Wine be good, the Experiment will succeed very well, insomuch that I remember I have made this way a Coagulum, from which no Liquor would drop down in about a minute of an hour. But whereas this great Naturalist conceives this hardning of the Egg's white to be perform'd by the heat of the Spirit

of

of Wine, I ſhall willingly confeſs he has aſſign'd the cauſe ingeniouſly, but muſt doubt whether he have done it truly: for there are diverſe things that ſeem to argue Spirit of Wine, as inflammable as it is, to abound with a piercing Salt, and that ſuch ſaline Corpuſcles may ſuffice to crudle whites of Eggs, our freſhly-mention'd Experiment of crudling the white of an Egge with Spirit of Salt does ſufficiently declare; and not only we have perform'd the like effect with ſome other Acid Spirits, and particularly that call'd Oyl of Vitriol, but it may be produc'd, though more ſlowly, ev'n by a crude Salt; for by long beating the white of an Egge with a lump of Allum, you may bring it for the moſt part into white cruds. So that if we will allow the coagulation we treat of to be performed by the Spirit of Wine as hot, it ſeems that that heat muſt be only ſuch as may be aſcrib'd to the active particles of ſaline bodies, which yet are commonly accounted rather cold than hot. But becauſe I ſomewhat doubt how juſtly they are reputed ſo, I will adde, that I did purpoſely for tryals ſake, take the Serum or Whey that is wont to ſwim upon mans Blood after it is cold and ſetled, and indeavour'd in vain to coagulate it with ſuch Spirit of Wine as would coagulate Eggs, and yet this whey will at leaſt as ſoon as (if not much ſooner than) whites of Eggs coagulate over a gentle heat of Embers; which makes it doubtful, whether the effect proceed not from the greater correſpondency in texture of the Spirit of Wine with one of the Liquors than with the other, rather than from the heat aſcrib'd to it, which did not at all coagulate the whey.

But although we have mention'd ſome Examples to ſhew that two fluid Bodies may be aſſociated into a conſiſtent one; yet we want not an Experiment to make it appear, that likewiſe by the change of Texture a fluid Body may be divided into two conſiſtent ones. This Experiment which we have partly taken notice of before (treating of Fluidity)

Fluidity) is, that having for tryals ſake by convenient degrees of fire diſtill'd over a due proportion of the more volatile parts of ſallet Oyl, neither the Liquor that came over, nor the ſubſtance that remain'd behind in the Retort was fluid, though the Oyl that yielded them had been ſo.

But when I put to the Oyl before Diſtillation a convenient quantity of common Salt, and one or two other things, that were fit to change the Texture of the branch'd or hookt Corpuſcles whereof it conſiſted; I could then obtain an Oyl of common Oyl, that both dropt into the Receiver in the form of a Liquor, and continued a Fluid Body; which may probably be of good uſe to Surgeons, Varniſhers, and Men of ſome other Profeſſions.

And to make it the more likely, that by Additaments of ſome ſuch nature as that newly mention'd, ſome groſſer and cloggy parts are retain'd, or elſe much ſubtiliz'd and otherwiſe altered. I ſhall adde that proſecuting a [illegible] I happen'd to meet with in the diſcourſe of a wand[illegible] Chymiſt, I practis'd a way ſo to defecate the dark muddy Oyl of Amber drawn *per ſe*, that a pretty pro[illegible] tion of it would come over ſo tranſparent and finely [illegible] lour'd, that the Experiment did not a little pleaſe tho[illegible] ſhew'd it to. And if it do not appear upon tryal, that this way of preparing Oyl of Amber does by detaining ſome parts, which though more groſs then the reſt, may yet be no uſeleſs one; impair the Remedy, and that it does not, upon ſome other ſcore infringe the medicinal vertue of the Oyl, the Experiment will not be unuſeful. For the Liquor that is thus prepared is not only very diaphanous and well colour'd, but ſo pure and ſubtle that 'twill ſwim, not only upon Water, but upon Spirit of Wine it ſelf. And 'twill be no deſpicable thing, if by extending or further applying this Experiment to other indiſpos'd Bodys, many Empyreumatical Oyls diſtill'd by

ſtrong fires in Retorts, can be brought to emulate eſſential Oyls (as Chymiſts call them) drawn in Limbicks, as to clearneſs and lightneſs.

The additament I laſt thought fit to make uſe of for purifying Oyl of Amber was briefly this. ℞. Two Pound or ſomewhat leſs of good Brandy, One Pound of good Sea-Salt, and half a Pound of the Oyl to be ſubtiliz'd, mix and diſtil them together.

Upon the mention I made above of the white Coagulum of the Spirits of Wine and Urine, I remember what I have ſometimes obſerv'd in the eſſential Oyl of Anniſeeds (as Chymiſts ſpeak) diſtill'd with ſtore of water in a Limbick and Refrigeratory, namely, that in the heat of Summer it would remain a perfect Liquor like other Chymical Oyls; but during the cold of the Winter, though they, notwithſtanding that ſeaſon, continued fluid as before, the Oyl of Anniſeeds would coagulate into a Body, though not of an uniform Texture to the Eye like Butter (but rather almoſt like Camphire) yet like it white and conſiſtent, not without ſome kind or degree of Brittleneſs.

And on this occaſion I will here inſert an Experiment which ſhould have been ſet down in that part of the former Hiſtory of Fluidity, where I mention, that the ſmall parts of a Body may be ſufficiently agitated to conſtitute a Liquor by the Air or other Agents not ſenſibly hot themſelves. The Experiment take thus. Caſting by chance my Eyes in the Winter time upon a glaſs of Oyl of Anniſeeds which ſtood coagulated by the cold of the ſeaſon, I preſently bethought my ſelf of making a Liquor (whoſe proceſs belongs to another Treatiſe) of which as ſoon as I had prepar'd it I made this Tryal. I melted with a gentle heat the congeled Oyl of Anniſeeds to make it flow, and then cover'd part of it in another glaſs with a Mixture I had provided: and having let them both reſt in the window,

I found, that the meer Oyl being fully refrigerated again, coagulated as before; but that which was cover'd with the other Liquor continu'd fluid both day and night, and in ſeveral changes of weather, and does ſtill remain at the bottom of the *Menſtruum* a clear Oyl diſtinct from it, though I have purpoſely ſhaken them together to confound them.

And becauſe, *Pyrophilus*, I have not diſcover'd to you the *Menſtruum* I made uſe of, I will here preſent you with a Succedaneous Experiment made with a common Liquor. I took then good clear Venetian Turpentine, and having ſlowly evaporated about a fourth or fifth part of it, till the remaining ſubſtance being ſuffer'd to cool would afford me a coherent Body, (or a fine Colophony) I caus'd ſome of this tranſparent and very brittle Gum (of which I have elſewhere taught you ſome uſes) to be reduc'd to fine powder: of which I put into pure Spirit of Wine a greater proportion, then I judg'd the Liquor was capable of diſſolving, to the end that when the Spirit had taken up as much of the Powder as it could, there might remain at the bottom a pretty quantity of our Colophony. On which, though the *Menſtruum* (being already glutted) could not act powerfully enough to diſſolve it, yet it might give the matter (which it had already ſo far ſoftened, as to reduce it into a coherent maſs) agitation enough to emulate a fluid (though ſomewhat viſcous) Body. And accordingly I obtain'd a ſluggiſh Liquor, which continued fluid, as long as I pleas'd to continue the *Menſtruum* upon it. The like Experiment I try'd with clarify'd Roſin, and with fine Colophony, though but bought at the Shops; and although the Tryal ſometimes ſucceeded not ill, yet I found not the ſucceſs conſtant and uniform, whether becauſe the Bodys to be diſſolv'd were not defecated and pure enough, or that I did not hit upon the beſt proportion between the Solvent and them. But this circumſtance I

ſhall not omit, that when the glutinous Liquor was ſeparated from the *Menſtruum*, it would by degrees, though but ſlowly, harden in the Air. The Application of which property, for the preſervation of ſmall and very tender Bodies, I ſhall not here more expreſly hint then by having barely nam'd it. I had forgot to adde, that whilſt the ſubſtance continu'd fluid, I could ſhake it, (as I lately told you I could the Oyl of Anniſeeds) with the ſupernatant *Menſtruum*, without making between them any true or laſting Union.

Which laſt circumſtance brings into my mind another Experiment that I likewiſe forgot to adde to that part of the former Hiſtory of Fluidity, where I take notice, that the particular Textures of fluid Bodies may be reckon'd among the chief cauſes of their being diſpos'd, or indiſpos'd to mingle with one another. For partly to confirm this Conjecture, and partly to manifeſt that 'tis not univerſally true which Chymiſts are wont to think, that Acid Salts and Oyls will not incorporate or mingle; I took an arbitrary quantity (and, as I remember equal weight) of common Oyl of Vitriol and common Oyl of Turpentine, as I bought them at the Druggiſts: theſe I put together very ſlowly, (for that circumſtance ſhould not be omitted) and obtain'd according to my deſire an opacous and very deep-colour'd mixture, whoſe almoſt Balſam-like conſiſtence was much thicker than either of the Liquors that compos'd it. (The like Experiment alſo ſucceſſfully try'd with ſome other Chymical Oyls, but found none preferable for this purpoſe to Oyl of Turpentine.) And to make it probable that the diſpoſition of theſe Liquors to mingle thus preſently together depended much on their Texture, we made the mixture be warily diſtill'd over, (for elſe the Experiment will ſcarce ſucceed) and thereby obtain'd, (as we elſewhere mention to another purpoſe) a certain groſs ſubſtance, which was that which

ſeem'd

ſeem'd to mediate the former union betwixt the two Liquors. For this ſubſtance being ſeparated, and thereby the Texture of one of the Liquors (or perhaps both) being chang'd, the Liquors (which came over very clear into the Receiver) ſwome upon one another; nor have I ſince been able by ſhaking them together to confound them for any conſiderable time, but they preſently part again, and do to this day remain diſtinct as well as tranſparent. But after having forgot to ſet down theſe things in their proper place, I muſt not forget alſo, that to employ here more words about them were to digreſs.

To this then annex we, that the Liquor we elſewhere mention our ſelves to have diſtill'd from Benzoin, has been and is ſtill ſubject to much more frequent viciſſitudes of Fluidity and Firmneſs; for part of it all the year long continues in the form of a blackiſh Oyl, and the reſt according as the ſeaſon of the year or of the day makes the weather cold or hot, frequently changes its Texture, ſometimes appearing perfectly the ſame with the newly-mention'd Oyl, and ſometimes ſhooting into clear and variouſly-ſhap'd Cryſtals, which faſten themſelves to the bottom and ſides of the Veſſel, till a warmer part of the day or of the Seaſon reſolves them again into a Liquor. And theſe two laſt Obſervations may alſo ſerve to confirm what we formerly taught, that the Fluidity of ſome bodies depended almoſt wholly upon the various agitation of their parts: for in theſe inſtances the parts of the Anniſeeds and thoſe of the Benzoin, upon the operation or abſence of the languid heat of the ambient air, ſometimes agitating them, and ſometimes ſuffering them to reſt, did conſtitute a fluid or a conſiſtent Body. And having thus taken notice of this upon the by, we will adde to the other Examples mention'd under this ſecond head, that which it afforded as to our preſent purpoſe by Salt-Petre, which being diſſolv'd in a ſufficient quantity of common water, will ſeem to be loſt

in

in it, and to conſtitute with it one uniform fluid ſubſtance; but if a competent quantity of that water be boil'd (or permitted to exhale) away, and the remaining liquor be ſuffered to reſt a while, eſpecially in a cool place, the ſaline particles will be re-uniting themſelves and by the excluſion of the aqueous parts, conſtitute ſtable and determinately-figur'd Ice-icles or Cryſtals.

The conſideration of this may ſuggeſt to us another way, that ſeems quite contrary to the former, whereby ſome bodies may become firm and ſolid, and that is by the intermingling of a due proportion of water or ſome other Liquor. For, though the ſmall parts of ſuch fluid Bodies, being themſelves in motion, are apt to give thoſe of others ſuch an agitation as we have formerly taught that Fluidity principally depends on; it ſeems that the admiſſion of any Liquor muſt rather conduce to the making of a body fluid than conſiſtent; yet if we conſult Experience, it will inſtruct us otherwiſe; for when I have taken either an equal or a double weight of Oyl of Vitriol and diſtill'd it warily from running Mercury; very much the greater part of the Liquor would come over, and leave behind it a very white Powder conſiderably fixt. And if we examine that familiar Production of Chymiſtry, *Mercurius dulcis* (which they now uſe to make by ſubliming of together two parts of crude Mercury, with but one of Sublimate, which conſiſts chiefly of Mercury already) we may find that in That which is counted the beſt, the fluid Body of Quick-ſilver is ſo contex'd with the Salts it carries up in Sublimation, that the dry and brittle Body they compoſe may contain far more (perhaps twice more) Quick-ſilver than Salt. And other Experiments may perſwade us, that the mixture of a convenient Liquor may cement bodies into one hard Concretion, which would ſcarce be compacted together otherwiſe. Nor is it againſt reaſon that it ſhould be ſo; for there may be differing qualifications

fications required to a body whilſt it is conſtituting, and when it is conſtituted, and though the motion of the parts that make it up, oppoſe the firmneſs of a formed body, yet it may conduce to the making of a firm body: for when a great many hard Corpuſcles lye together looſe and incoherent, they do, as we formerly noted, emulate a fluid body; whereas by the mixture of a Liquor, thoſe looſe Corpuſcles being for a while diſſociated and put into motion, they may after many Evolutions apply themſelves to one another after that manner that is moſt requiſite to make them touch one another cloſely, and according to a greater ſurface. Whereupon it often follows, that the Liquor in which they did formerly ſwim is either ſqueez'd out upon their cloſing, or elſe ſo diſpers'd in ſmall particles, and diſpos'd of among thoſe of the harder Corpuſcles, that they are unable to agitate them, or prejudice their mutual coheſion.

And here to dilucidate the ſubject under conſideration by an inſtance that ſeems very pertinent to it, we will make a further uſe of the Experiment formerly mention'd touching the burning of Alabaſter: For if the powder, after it has done boyling and has been ſufficiently burnt, and kept ſome hours (the moſt experienced Artificers obſerving that it is not ſo convenient to employ it preſently after it is taken off the fire) be well beaten and tempered up with fair water almoſt to the conſiſtence of thin pap, if the powder have been rightly prepar'd and ſkilfully temper'd, you ſhall ſee that fluid ſubſtance in a few minutes of an hour begin to ſet (as the Trades-men ſpeak) that is to exchange its Fluidity for Firmneſs. ſo that if it were before caſt into a mould, it will perfectly retain the figure of the internal ſurface thereof.

Now that in our mixture there is for a while ſuch an agitation of the hard parts produc'd upon the affuſion of the water, and afterwards an excluſion of the ſuperfluous wa-

ter,

ter, we may confirm partly by this, That when any conſiderable quantity of burnt Alabaſter is temper'd up with water, the mixture after a little time grows ſenſibly hot, and ſometimes continues ſo for a pretty while: and partly alſo by this, That having purpoſely for tryals ſake fill'd a new and good Glaſs-Vial, containing about half a pint, or pound, with the mixture we ſpeak of, and when it was top full, ſtop'd it up very cloſe, the liquid mixture within leſs than half an hour crack'd the Vial (though ſtanding in a window) in ſeveral places, and at thoſe creviſes diſcharg'd it ſelf of about a ſpoonful of clear water, the remaining mixture retaining perfectly the figure and dimenſions of the Vial, and growing as hard as Chalk or ſomewhat harder, inſomuch that we were fain to imploy ſeveral ſtrokes with a ſtrong Iron to divide the maſs.

And let me here adde, that ſome other ſubſtances may this way afford much ſolider Bodies than burnt Alabaſter does: and therefore it may be a thing of good uſe to enquire out and try what other Bodies, eaſily to be procur'd, may be thus brought to a new and laſting Solidity. For the Learned Hydrographer, *Fournier*, ſpeaking of thoſe Damms or *Digues* (as he calls them in his Language) which are ſometimes made in the Sea to ſecure Shipping, (as I have ſeen at the Port of *Genoa* and elſewhere) after having told us that the Romans made the faireſt Harbours in the World by the help of a certain Sand to be met with at *Cuma* and *Puteoli* in the Kingdome of *Naples*, which Sand mingl'd with a third part of Quick-lime acquires in the water a flint-like hardneſs; ſubjoyns this Obſervation of his own, *J'ay veu*, &c. that is, I have ſeen (ſayes he) in *Flanders* near *Tournay* a certain ſort of aſhes of Lime made of Marble, which was excellent for any kind of work made in the water. For having made a Bed of great ſtones, they caſt upon them whole Baggs full of ſuch aſhes inſtead of Mortar,

Hydrograph. Du P. G. Fournier, Lib. 2. Cap. 6.

tar, and the water betwixt the ſtones having temper'd up theſe aſhes, petrify'd them to that degree, that in a ſhort time they became as hard as Marble. Thus far He. But to purſue our former Diſcourſe.

That alſo which we intimated of the conduciveneſs of the various tumblings to and fro of the hard particles to their uniting into one firm concretion, ſeems confirmable by what we have obſerv'd in ſome ſaline Liquors, eſpecially certain parcels of Spirit of Harts-horn, which whatever were the conſtitution of the ambient air, remain'd fluid ſome of them for many months, after which the ſaline Corpuſcles began to ſhoot at the bottom of the remaining Liquor into exquiſitely-figur'd Cryſtals, which at length grew copious enough. For this ſpontaneous coagulation of the little ſaline Bodies happening ſo late, it ſeemed that it was preceded by almoſt innumerable evolutions, which were ſo many and ſo various, that at length the little bodies came to obvert to each other thoſe parts of themſelves by which they might be beſt faſten'd together and conſtitute a firm body. Which conjecture ſeem'd the leſs improbable, becauſe we could not well imagine that this coagulation proceeded (as that of diſſolv'd Allum and other Salts is wont to do) from the evaporation of the ſuperfluous Liquor; for the Glaſſes wherein what we have mentioned happen'd being carefully ſtop'd, there was no danger of ſuch an avolation, and if any thing could get away, it muſt have been the ſubtil peircing and fugitive Spirit, (which indeed, as my Noſe had inform'd me, does oftentimes penetrate ordinary ſtopples) for the flying away of thoſe volatile parts would only have left the remaining Liquor more aqueous. And 'tis well known to thoſe that deal with ſuch kind of Liquors, that the more aqueous they are, the leſs apt they are to Cryſtallize. And however it will ſerve our turn, that there was but an inſenſible diminution of the Liquor upon the

receſſe of whatever it was that got through the Cork.

To the ſame purpoſe I remember alſo, that having in a Cryſtal Vial carefully kept a pretty quantity of well-colour'd Tincture of Amber, made with pure Spirit of Wine, it remain'd fluid for a year or two, and during that time preſented us with a ſtrange Phænomenon that belongs to other papers. * But having been abſent for two or three years from the place where we lock'd it up, we found, when we came again to look upon it, that that though it had formerly remain'd fluid ſo long, yet ſeveral yellow lumps of Amber, almoſt like Beads, with one ſide flat, had here and there faſten'd themſelves partly to the bottom, and partly to the ſides of the Glaſs: the reſt of whoſe internal ſurface continues yet tranſparent.

* This Phænomenon is partly deſcrib'd at large in one of the Authors Phyſico-Mechanical Experiments.

Another thing whereby bodies become ſtable is, the admiſſion of adventitious Corpuſcles into their Pores and receſſes. And of the wayes by which theſe foreign Corpuſcles may bring the ſubſtance they invade to be compact, theſe four appear the chief.

Firſt then, the adventitious Corpuſcles we ſpeak of may produce ſtability in the matter they pervade, by expelling thence thoſe voluble particles which, whilſt they continu'd in it, did by their ſhape unfit for coheſion, or by their motion oppoſe the coalition, or diſturb the Reſt of the other particles whereof the Body conſiſted. But of this having already diſcourſed, proceed we to what is to follow.

In the next place then, foreign Bodies may contribute to the ſtability of a ſubſtance they get into, by hindering the motion of the little Bodies that conſtitute it.

And thirdly, ſuch advenient Bodies, eſpecially if they be not of the ſmalleſt ſize, may produce a firmneſs in the ſubſtance

ſtance which they get into, by conſtituting with the particles it conſiſts of, Corpuſcles more unapt for motion, and fitted for mutual coheſion.

Theſe two we mention together, becauſe that very often Nature imploys them together for the introducing of ſtability into Matter.

To theſe ſeems to be reducible the way of turning the fluid body of milk into cruds by the mixture of a little Runnet, whoſe ſaline particles pervading the body of the milk, do not only make a commotion in the parts of it, but faſten the branched particles of it to one another, and with them conſtitute a body of another texture than was the milk; and the weight of theſe crudled bodies reducing them by degrees into a cloſer order, does, whilſt it preſſes them together, ſqueeze out the thinner and more ſerous Liquor, which the Runnet was unable to coagulate, and which being thus ſever'd from the groſſer parts of the milk, may well be more fluid than milk it ſelf is wont to be. And that there is ſome coalition of the particles of the Runnet with the coagulated ones of the milk, may appear by the complaints that Houſwives ſometimes make of their Dairy-maids, that the Cheeſes taſt too ſtrong of the Runnet, when too great a proportion of it has been mingled with the milk. And though we aſcrib'd the crudling of the milk to the ſaline particles of the Runnet, we ignore not that not only common Runnet, but alſo diverſe juices of herbs will crudle milk, as is well known in thoſe parts of *Italy* where Cheeſe is made without Runnet. But we made eſpecial mention of the ſaline Corpuſcles of the Runnet, becauſe really Houſwives are wont to ſalt it, and becauſe ſaline Liquors do manifeſtly and powerfully operate in the coagulation of milk, which may be crudled by juice of Limons, and I know not how many other Acid Salts. And to manifeſt yet further the coagulative power of them, we have ſometimes in about a minute of

an hour arrested the Fluidity of new milk, and turn'd it into a crudle substance, only by dexterously mingling with it a few drops of good Oyl of Vitriol. But of the effects of various Salts upon milk we elsewhere may, and therefore shall not now, discourse.

Between this last recited Experiment, and the two following ones, 'twill not be improper to insert the immediately ensuing one, for the Affinity which it seems (in different respects) to have with both.

I remember (then) that I divers years ago prepar'd a Salt, which either was, or at least answer'd well to the qualities ascrib'd to that which is now called *Glauberus*'s *Sal Mirabilis*, which seem'd to have in it a coagulative power, in reference to common Water. For whereas Salt of Tartar, Common Salt, Nitre, *&c.* being dissolv'd in Water, do upon evaporation of a sufficient quantity of that Water, recover indeed their pristine Saline Forms, yet they do but coagulate themselves, without concoagulating with them, either any Water, or at least so much, as Chymists have thought worth the taking notice of: Whereas this Salt we speak of, being prepar'd for the purpose, and dissolv'd in a convenient quantity of Water, does upon its recoagulation so dispose of the aqueous Particles, among its own Saline ones, that if the Experiment be well and carefully made, almost the whole mixture will shoot together into fine Chrystals that seem to be of an uniform Substance, and are consistent enough to be even brittle, and to endure to be pulveriz'd, sifted, *&c.* though the Concretion may have such a Proportion of Water in it, that (as I remember) when the Experiment succeeded well, from three parts of Water and but one of Salt, I had about four parts of Crystals.

I need not tell you that this Salt seems to have a somewhat more then ordinary Resemblance of a true Coagulum, since it reduces so much water into a stable consistence;

yet

yet it does in no contemptible proportion materially concur to the Body produc'd. But I may hereafter (which I muſt not do now) entertain you about a Salt of a differing kind from this; and which put me upon conſidering, whether there may not be a Coagulum more properly ſo call'd of Common Water, which may in a very ſmall proportion operate upon a great quantity of that Liquor, as Runnet does on Milk.

I have not yet examined whether it will be ſufficient to refer meerly to the ſecond and third ways lately mention'd of making Bodies become ſtable in the Phænomena I am about to ſpeak of, or whether it may be reaſonably ſuppos'd (and added as a fifth way) that the Bodies to be coagulated may (in great part) be brought to be ſo; by ſo acting upon the Bodies to which they are put, that the Agent Liquor (if I may ſo ſpeak) does by its action comunicate to the ſubject it works on, or loſe upon ſome other account ſome ſubtle parts whoſe abſence fits the diſpos'd remaining Fluid for ſuch a Coheſion, as may ſuffice to make a Body be (though very ſoft, yet) conſiſtent. But however 'twill not be amiſs to take ſome notice of Effects, which, what e're the cauſe be, belong to the Hiſtory of Fluidity and Firmneſs.

I ſome years ſince prepar'd a Subſtance of a whitiſh colour, which would not only deſtroy the Fluidity of ſome other Liquors, but would give a conſiſtency to a notable proportion of Oyl of Vitriol it ſelf, though the parts of this Liquor be preſum'd, upon the ſcore of its corroſiveneſs, and its aptneſs to grow very hot with many other Bodies and make them ſmoke, to be very vehemently agitated.

And I remember that I ſometimes ſhew'd the curious a Glaſs Vial well ſtopt, upon the bottom of which lay a little of this newly mention'd whitiſh powder, over which there was a conſiderable proportion of Oyl of Vitriol in a

cur-

conſiſtent Form without ſeeming to have any thing to do with the Powder, as indeed it had been only pour'd upon it, and ſuffer'd to ſtand in the cold for ſome time (which if I miſtake not was a day or two) at the end of which the above mention'd change was wrought on the Liquor by the powder which did not appear to be diſſolv'd thereby. Which Phænomenon ſeem'd indeed to argue, that there happen'd in this Experiment (that was not the only one of the kind I then made) ſomething like the coagulation formerly mention'd of Quickſilver by the Vapour of Lead, ſome ſubtle parts of the Coagulator, if I may ſo call it, inviſibly pervading the Liquor whoſe Fluidity was to be ſuſpended, though it ſeem not improbable to me, that the effect produc'd might depend upon both cauſes, this newly expreſs'd, and the other a little abovemention'd; where I gueſs'd that a change of Texture, and thereby of Conſiſtence in the *Menſtruum*, might be the reſult of the Operation of the *Menſtruum*, and the Body it acts upon. And becauſe this powder is not ſo eaſie to be prepar'd, I ſhall adde that you may (though not ſo well as by the newly mention'd way) ſee the Coagulation of a *Menſtruum* upon a firm Body which it does not ſeem to diſſolve by the enſuing Experiment, Take Cryſtals of Salt-Petre very well dryed, but not powder'd, and gently pouring on it in a Glaſs Vial ſome good Oyl of Vitriol till it ſwim about half an Inch, or perhaps more above the Salt, leave the Vial clos'd with a cover of Paper in a cool quiet place, where it may not be ſhaken; and if the Tryal ſucceed with you as did it with me, the Liquor will, though ſlowly, ſo ſettle it ſelf about the Nitre, that though you incline the Vial to any ſide (or perhaps turn it upſide down) it will not run out; and I have ſometimes taken notice of little Saline Bodies, and as it were Fibres, that ſeem'd to keep the parts of the mixture united together. I made alſo ſome other Tryals to coagulate unflegmatick *A. F.* upon Nitre and

and ſome other Bodies, the Phænomena of which Tryals, did not oblige me to renounce the lately mention'd Conjectures about the cauſes of ſuch changes of conſiſtency in Liquors, as I have been ſpeaking of: For I ſtill think it highly probable that the beſt Coagulator I have met with acts but as a finer ſort of Runnet, which in an inconſiderable quantity really diſperſes material parts of it ſelf through the Liquor to be wrought on, though theſe when the Coagulator is a conſiſtent Body, be perchance ſo few or ſubtle as not to make any Viſible diminution of the Body it parts with.

A more eminent Example to our preſent purpoſe may be afforded us Sometimes (for I am ſure the Experiment will not Always ſucceed) by the notable way of coagulating Quick-ſilver, and thereby turning it from a fluid into a firm body by the vapour of melted Lead, in which when it is taken off the fire (but before it be quite grown hard again) a little cavity muſt be made with a pebble or a ſtick, that the Quickſilver tied up in a rag may be nimbly put into that hole, and be congeal'd by the permeating ſteam of the cooling lead. Which Effect may be leſs hopefully expected by the way wont to be preſcrib'd by Authors (moſt of whom I doubt never made tryal of it) then by another that I have practis'd and may on another occaſion ſhew you. And that ſome metalline ſteam does really invade the Quickſilver, ſeems probable by the waſting of Lead by fuſion, and by the operations aſcrib'd by Chymiſts to the fume of Lead upon Gold, about which I may elſewhere tell you what is come to my Knowledge.

And I remember that not long ſince, an ingenious Phyſician of my acquaintance keeping ſome Lead long in fuſion to reduce it *per ſe* into a Calx, and holding his head often over the melting pot to obſerve the alterations of the metal, was ſuddenly purg'd diverſe times both upwards and downwards, which both he and I aſcrib'd to the Saturnine

turnine exhalations. And though I ſuſpected the Congelation formerly-mention'd might proceed from the egreſs of ſome ſubtil ſubſtance that formerly agitated, but after deſerted, the Mercurial Corpuſcles; yet that the Concretion of the Quick-ſilver might be effected by ſome benumbing vapour of the Lead, ſeems confirmable by a notable accident that befel that famous Geometrician Doctor *Wallis*, who related it to me as a Phænomenon he knew not well what to make of; namely, That he and other Learned Men at *Oxford* being minded to make the Experiment under conſideration, they found that upon the firſt fuſion of the Lead the immerſed Quick-ſilver was very well coagulated by it; but when they came to melt it the ſecond time, and put new Quick-ſilver into it, the Experiment would not ſucceed, at which they wonder'd, finding by Tryals that the Lead might be ſo eaſily deprived of its power of hardning Quick-ſilver. That this Obſervation will always hold true, I am not apt to believe; but that ſuch Learned and Candid Naturaliſts ſhould either be miſtaken in making it, or miſ-relate it, 'twere injurious to ſuſpect: wherefore ſuppoſing that to have then at leaſt happen'd which one of them regiſtred in writing, and more then one of them told me; it ſeems to countenance what we have deliver'd, and looks as if according to our Doctrine there were in Lead a coagulative ſteam or Spirit, and yet (at leaſt in that parcel of metal) in ſo ſmall a proportion, as that it almoſt totally diſlodges or ſpends it ſelf upon the firſt opportunity it meets with of paſſing into Quick-ſilver.

We have elſewhere to another purpoſe mention'd our having ſometimes (for, as we there advertiſe, it will not always ſucceed) made an Experiment which ſeems of kin to the former, and may give much light to the matter under conſideration, and it was this; We pour'd upon *Aqua fortis* common Sallet Oyl, which floted together at the top of

of it, but after ſome hours had its texture ſo changed by the aſcending ſteams or other ſubtle inſinuating particles of the ſaline Liquor, that it was turn'd into a white conſiſtent (and ſometimes a brittle) body like Butter, remaining all in one Cake on the top of the *Menſtruum*. And the like Experiment (but in a longer time) we have perform'd with expreſs'd Oyl of ſweet Almonds inſtead of common Oyl.

And to ſhew further, how much the operation of the ſame viſible Agent may be diverſified as to the Production of Fluidity or Firmneſs, according to the differing diſpoſitions of the Bodies it acts on. I have ſometimes (I remember) taken the *ſame Aqua Fortis*, or Spirit of Nitre wherewith I had coagulated expreſs'd Oyl of Olives, and having pour'd it off from the Butter-like Subſtance: I caſt into it ſome good Camphire, which without heat was thereby reduc'd into an Oyl, that retain'd a diſtinct Superficies from the *Menſtruum* which it ſwam upon, and would not incorporate with, ſo that the ſame numerical *Menſtruum* without the help of any degree of fire, turn'd a brittle Body into a Liquor, and the Liquor into a brittle Body (for ſuch is the Subſtance that may be made of common Oyl, if it be ſuffer'd to float long enough upon the coagulating Liquor) which brittle Subſtance (to adde that upon the by) ſeem'd to have receiv'd a more durable alteration from the ſteams of the *Menſtruum* then was expected. For not only when melted with fire, it would upon refrigeration recover its conſiſtence without becoming again fluid, as when 'twas in the form of Oyl; but I made a Tryal or two without ſucceſs to reduce it to a Liquor by mixing it with Oyl of Tartar *per Deliquium*, which, you know, has a great Faculty to find out and mortifie Acid Spirits, ſuch as thoſe of the Nitre or *Aqua Fortis* that had (whether as meer Acids I now examine not) coagulated our Oyl.

The fourth and laſt way whereby Corpuſcles entring from without into a Body may give it a ſtable conſiſtence, is by making ſuch a commotion in the parts of it, as may make them apply themſelves one to another according to a greater ſurface, or otherwiſe complicate and diſpoſe them after the manner requiſite to make them ſtick together.

This way of making Bodies become conſiſtent, is ſeldom or never employ'd by Nature without the concurrence of ſome of the other ways already mention'd: but we have diſtinguiſh'd it from the two laſt recited, becauſe in them we ſuppoſe that ſome of the adventitious Corpuſcles are ſtop'd in the body to whoſe firmneſs they conduce, and (though perhaps but in a very conſiderable proportion) do concur to make it up; whereas here we ſuppoſe that without materially concurring to conſtitute the body they work upon, they do only agitate and variouſly move the particles it conſiſts of, perhaps breaking ſome, bending and twiſting others, and in a word ſo altering the Texture, that the parts that did formerly either move ſeparately, or adhere together but looſly, are now reduced to a cloſer order, or a more implicated Texture, and thereby more firmly connected to one another. That the bare diſpoſition of the parts of a body in reference to each other, without any addition of forein matter, may do much towards ſtability, we may ſee both in ſome examples formerly mention'd, and in Oſier wands, which when lying looſly in an heap together may each of them very eaſily be diſſociated from the reſt; but when they are breaded into a Basket, they cohere ſo ſtrongly, that when you take up any one of them, you ſhall take up all the reſt. To which may be added thoſe many obvious though perhaps unheeded Inſtances wherein by the bare Texture of the ſlender hair or filaments whereof Wool or Silk conſiſts, Cloth, Silk-ſtockins, and many other durable Gar-

Garments are made by illiterate Tradeſmen.

We may alſo obſerve the force of bare motion in altering the texture, and thereby the conſiſtence of bodies by the common way of Churning, for there the external impulſe of the Churn makes a great commotion in the parts of the Cream, and tumbles and ſhuffles them perpetually to and fro among themſelves, whereupon it happens, that the more branched Corpuſcles meeting with one another are intangled, and thereby ſeparated from the reſt, and after many occurſions all theſe parts are at length faſten'd to one another, and excluding thoſe of the Butter-milk, which ſeem not ſo conveniently ſhap'd for mutual coheſion, do conſtitute Butter; which is wont to be made yet more conſiſtent, or rather more compact, by being beaten or otherwiſe compreſs'd, as the parts thereby reduced into a cloſer order ſqueeze out the fluid Butter-milk that was intercepted among them.

It will perhaps be thought more ſtrange that a fluid Body, nay a diſtill'd Liquor which is very volatile and paſſes for ſimple and Homogeneous, and is at leaſt far leſs compounded than milk, ſhould by motion, without the mixture of any new matter, be made coherent: and therefore I hope that it will not only ſecond the Example newly alleg'd, but likewiſe confirm ſome main points of our Doctrine touching Firmneſs, if we obſerve that ev'n the Chymical Oyl of Turpentine, which paſſes for one of the Principles or Elements of that Body, may be in great part, if not wholly, coagulated without addition. And yet (not to anticipate what I may have occaſion to deliver elſewhere concerning this Experiment) I ſhall now only relate, That enquiring a while ſince of a very expert Chymiſt, whether he had not ſometimes obſerv'd (which I have often done, as I elſewhere declare) Oyl of Turpentine to begin to coagulate if it were often diſtill'd; he went with me to his Laboratory, and there let me ſee in a Receiver ſome Oyl of Turpentine

which he had often diſti'l'd over *per ſe*, in good part coagulated into a whitiſh and conſiſtent Body: affirming alſo to me, that he had ſometimes by frequent Diſtillations, without Addition, obtain'd from clear Oyl of Turpentine a far greater proportion of ſuch a ſtable ſubſtance. Whoſe conſiſtence, whether it ſhould be aſcrib'd to the fires breaking the Oyly Corpuſcles into parts more fit for mutual coheſion, or whether it proceed from a new texture of the ſame Corpuſcles, only chancing by thoſe various Evolutions to be diſpos'd after ſuch another manner as to complicate or otherwiſe connect them, I need not now ſpend time to enquire; ſince 'tis enough for my preſent purpoſe, that in this example we have one that declares, how much ev'n motion without the Addition of any ſenſible ſubſtance may in ſome caſes conduce to Firmneſs.

And here to illuſtrate our Doctrine about this Quality and Fluidity, by ſhewing what the inteſtine motion of the parts, even without the aſſiſtance of adventitious heat, may do, to make a Body change its conſiſtence according to the previous diſpoſition of the matter, and become of firm, fluid: as we lately ſaw Oyl of Turpentine made of fluid, firm. I will adde on this occaſion what I obſerved of Oyl of Wax diſtilled in a Retort with an Additament of the like nature with that I formerly mentioned, when I ſpoke of the fluid Oyl drawn from Oyl of Olives. For this Oyl of Wax, though at firſt it came over for the moſt part, if not totally, in the form of a Butter; yet by ſtanding on a ſhelf (and that not in a hot place, as a Stove or Laboratory) I obſerved it little by little to reſolve into a tranſparent Oyl, and purpoſely enquiring of him that lookt after it, whether or no this effect might not be aſcribed to the increaſed warmth of the Weather, he aſſured me of the contrary, having taken notice what effects the changes of Weather had upon it.

But what if we ſhould ſay, that fluidneſs and ſtability depends

depends ſo much upon the texture of the parts, that by the change of that texture the ſame parts may be made to conſtitute either a fluid or a dry body, and that permanently too? Theſe laſt words I adde, becauſe of what may be ſaid to this purpoſe concerning the change of water into Ice, and Ice into water, and of metals into fluid or hard bodies, by fuſion and refrigeration: for in theſe examples the acquired hardneſs of water and fluidity of metals may be preſently loſt upon the bare removal of thoſe bodies into a temperate air; whereas in the inſtance we are to give, the acquired texture is ſo durable, that without an extream external violence, ſuch as would deſtroy moſt other ſtable bodies, it is not to be deſtroyed. And this inſtance is afforded us by that admirable Repoſitory of Natures wonders, Quick-ſilver: for if ſome Ounces of this fluid mineral be put into a convenient glaſs veſſel, and that veſſel be firſt exactly ſtop'd and kept for 6, 8, or 10 weeks (or longer, if need be) in a ſand Furnace whoſe heat may be ſtrong and conſtant, the Corpuſcles that conſtitute the Quick-ſilver will, after innumerable revolutions, and perhaps bendings, twiſtings, and other changes, be ſo connected to one another, that inſtead of a fluid Body, they will appear in the form of a red powder, that Chymiſts Precipitate *per ſe*: which change is ſo unexampl'd, that though among the more curious Spagyriſts it be very well known, yet many Naturaliſts cannot eaſily be brought to believe it; whom to convince of the poſſibility of it by a much leſs tedious preparation, I take half a pound or a pound of Quick-ſilver, and with a ſtrong fire diſtil it out of a glaſs Retort, and for the moſt part there will remain in the bottom and about the ſides of the veſſel a little red powder, which ſeems to be nothing but part of the fluid body (moſt expoſed to the action of the fire) turned into a dry one in eight or ten hours ſpace.

After what manner the fire produces ſo odde a change

in

in the Quick-ſilver, I do not preſume to know. 'Tis true, that though the parts of Liquors do, as we have formerly taught, touch one another but in part of their ſuperficies, yet they all of them ſeem to have ſome degree of viſcoſity, or ſome ſlight and looſe complication or other kind of Adheſion of parts, as appears by their being ſo eaſily contexed into thoſe thin membranes or films we call bubbles, inſomuch that not only Spirit of Wine, that ſeems the moſt light, and moſt fluid of Liquors, will afford bubbles, but (what may ſeem ſtrange) we have divers times purpoſely obſerv'd, that Quick-ſilver it ſelf, as ponderous as it is, eſpecially being ſuffer'd to fall in a ſlender ſtream into a veſſel almoſt full of the ſame mineral, will afford bubbles numerous and large enough, although (as thoſe alſo of the Spirit of Wine) quickly vaniſhing.

And hence it might be imagin'd, that in the operation we are treating of ſome ſuch change is made in the Quick-ſilver, as we formerly obſerv'd to be made in the white of a Egg, when by a new diſpoſition of its parts, either heat or beating it makes it a kind of ſtable body, or elſe it might be pretended, that there is a variety of parts argu'd to be in Quick-ſilver by the great variety of its effects upon other bodies, and that by the frequent evolutions which the fire makes of thoſe parts among themſelves, they come at length to be ſo appli'd to one another, that either they lock into each other as it were, or ſlip upon one anothers ſurface in ſuch a manner as that as much of their ſurfaces immediately touch one another as is requiſite to make them cohere, as we formerly mention'd of ſeveral very ſmooth pieces of glaſs mutually adhering without any other Cement than the congruity and immediate contact of their ſurfaces. But though theſe Conjectures and divers others might be propos'd, yet I fear all of them would prove but meer Conjectures. Nor were we much aſſiſted to make better by looking upon our Mercurial precipitate in one of the

the beſt Magnifying Glaſſes in the World; for what we there diſcover'd was only, that the red powder had in it many Corpuſcles of ſundry other colours, and that the little grains of powder ſeem'd to be of no determinate ſhape, but look'd like ſlender fragments of Red Coral: and having but ſome ſmall duſt of a ſhining precipitate of Gold and Mercury into the ſame Augmenting Glaſs, all we could diſcern was, that the little grains of this precipitate differ'd from thoſe of that made of Mercury alone, in that theſe (which a Chymiſt would take notice of) were ſo tranſparent throughout, that one would verily think he beheld the beſt ſort of thoſe precious Stones-Gold-ſmiths call Granats. But though we pretend not to make out how the new Texture is produc'd in the Quickſilver, yet to make it ſtill more evident that its change of conſiſtence proceeds from its change of Texture, we will adde, that having a great curioſity to try whether our powder could not be made fluid again, I procur'd ſome precipitate *per ſe* of a Perſon who formerly lived with me, and was expert in many Mercurial operations, and preſented me ſome of his own making: this being weigh'd and put into a convenient glaſs was carefully preſs'd with a naked fire (which ſhould be ſtronger than that wherewith it was precipitated) and at length it roſe by degrees in fumes, which ſettl'd in the neck of the Glaſs in many drops of reviv'd running Mercury; all which being collected into one, we found that there wanted but about a ſixth or ſeventh part of what we had put in, and we ſuppos'd we ſhould not have wanted that neither, but that the vehemence of the fire had melted the glaſs, which ſwallow'd up a part of the powder that made a great ſhew through it, after what was colliquated had been remov'd from the fire.

This Experiment brings into my mind another that was judg'd uncommon enough, and it was This: Being not long ſince maſter of about half an ounce of a certain Mercury,

cury, which ſome ways of examining it that I had employ'd, induc'd me to think Mercury of Saturn; I imagin'd (for ſome reaſons) that it might be made very ſerviceable to confirm our Doctrine touching Fluidity and Firmneſs. And accordingly I found upon Tryal, that I could, barely by ſhaking it long, reduce it to a black powder: in which form it would continue as long as I pleaſe to let it do ſo. And when to the By-ſtanders there appear'd nothing in it that gave ſuſpition of a fluid Body, I could in a Trice, only by dexterouſly rubbing it in a ſmall Marble Mortar, reduce it little by little into running Mercury, as it had been before. Which quick paſſage from one quality to another, being made, not only without the help of Fire, but without adding or taking away any viſible Subſtance, prov'd no ignoble Inſtance, how much Motion and Reſt, and the thence eaſily reſulting Texture of the Component Corpuſcles of a Portion of Matter, may contribute to its Fluidity or Firmneſs.

From the Experiment of precipitating Quick-ſilver *per ſe*, and from ſome other things, partly deliver'd already, and partly to be deliver'd by and by, we may learn what to think of the opinion of ſome Eminent Modern Philoſophers who teach, that a fluid body is always diviſible into bodies equally fluid, as Quantity into quantities, as if the particles of fluid Bodies muſt alſo be fluid themſelves: for by them it ſeems to appear, that Quick-ſilver, and ſome other other actually fluid Bodies conſiſt very much of hard Corpuſcles, ſince by the change of their Texture they may be deprived of their Fluidity and become ſtable. We ſee alſo that the ſtiff and ſolid particles of Salts diſſolv'd in common water, and of Silver diſſolv'd in *Aqua fortis*, being by thoſe Liquors ſufficiently diſſociated and ſeparately agitated, do with them conſtitute fluid Bodies. And we have elſewhere mention'd to another purpoſe an Experiment which may not impertinently

tinently be repeated here, namely, that by putting together into a glaſs Retort one part of Quickſilver and four of common Oyl of Vitriol, and diſtilling them in a ſand Furnace with a ſtrong fire, there remain'd in the bottom of the Veſſel a ponderous Calx or Powder, ſo far from being fluid, that it was but in part diſſoluble in water: and that which ſeems to prove that in the very liquid Oyl of Vitriol, though a diſtill'd Liquor, the ſaline Corpuſcles that chiefly compoſe it, do retain their ſtiffneſs (generally to be found in undiſtill'd Salts) is, that by ſteeping our Calx in fair water, we could ſeparate from it a conſiderable quantity of particles, which upon the evaporation of the water coagulated into ſtore of ſaline and brittle bodies. And that theſe proceeded rather from the *Menſtruum* than the metal, we were induc'd to think, by obſerving the dry Calx, before any water was pour'd on it: for though the ſaline part of the Mixture did not weigh (perhaps any thing near) ſo much as the Mercurial diſtinctly did, yet the Aggregate or Mixture did weigh a great deal more than the Quickſilver did when it was put in; and the Oyl of Vitriol that was abſtracted, a great deal leſs than it did before it was committed to diſtillation. Nay, I once or twice obſerv'd in a glaſs, where I kept a quantity of Oyl of Vitriol, that there did ſpontaneouſly faſten themſelves to the ſides little ſaline Cryſtals, which when I took out, I found hard and brittle; but when I had for tryal ſake expos'd them to the air, they preſently reſum'd a fluid form, and appear'd to be Oyl of Vitriol. In the Obſervation alſo lately mention'd concerning the ſpontaneous coagulation of Spirit of Hartshorn, it ſeems evident, that Bodies which are all or moſt of them hard, and appear ſo when they are commodiouſly connected to each other, may yet conſtitute a fluid body when they are reduc'd to ſufficient ſmallneſs, and put into a convenient motion. And indeed, if the leaſt par-

ticles of fluid bodies were not (many of them at leaſt) indowed with their determinate bigneſs and ſhapes, but that ſuch fluid bodies could be always divided into particles fluid alſo, how comes it to paſs that ſome Liquors cannot pierce into or moiſten ſome bodies which are eaſily pervious to other Liquors? for if the particles of the excluded Liquor were of neceſſity always diviſible into fluid ones, there ſeems no reaſon why they ſhould not be ſub-divided into ſo very ſmall ones, as that no pores can be ſuppos'd little or odly figur'd enough to keep them out.

'Tis true indeed, that as it is hard to demonſtrate, ſo it is not eaſie to diſprove, that the matter whereof fluid bodies conſiſt is capable of being indefinitely divided: and it may be granted too, that by how much the ſmaller parts a body is divided into, by ſo much the more eaſily, *Cæteris paribus*, are the parts of that body to be put into motion. But this diviſibility of a fluid body into perpetually leſſer and leſſer parts belongs not to it properly as it is Fluid, but as it is a Body; ſuch diviſibility, if ſuppos'd true, being a primary affection of matter it ſelf, and belonging as well to thoſe portions of it that are hard as to thoſe that are fluid. And though it were admitted, that ſuch an endleſs diviſion as is preſum'd might be made Mentally (as they ſpeak in the Schools) that is by the thought or operation of the mind, yet it would remain a great queſtion whether or no Nature does actually ſo far mince and ſub-divide Bodies: as may appear by what has been freſhly noted. And however, it is not only requiſite to the conſtitution of a fluid body that the parts of it be ſmall enough, but that they be alſo actually mov'd. For we obſerv'd not long ſince, that the duſt of Alabaſtar put into motion did (though its Corpuſcles were not inſenſible) emulate a fluid Body, and immediately ceas'd to be fluid when they ceas'd to be agitated: whereas the particles of water, as minute and apt as they are to conſtitute a fluid ſubſtance, do yet make

make that hard and brittle body we call Ice, when thoſe little particles upon what account ſoever are reduced to be at reſt.

By what has been hitherto diſcours'd, we may alſo be aſſiſted to judge of the Doctrine of the Chymiſts, who teach that in all Bodies, Coagulation, Stability, Hardneſs and Brittleneſs depend upon Salt: for though what above has been ſaid of Crudling of milk by ſaline Liquors, and the hardneſs and brittleneſs obvious in Salts themſelves, may keep us from denying that the ſaline principle is very powerful in the coagulation of ſome bodies, and does produce much firmneſs or even brittleneſs in many or moſt of the concretes wherein it is predominant; yet this hardning power of Salt ſeems not to proceed from any peculiar and inexplicable property it has to coagulate other bodies or make them compact, but from the ſhape and motion of its Corpuſcles, which it ſeems are more fitted by Nature than thoſe of many other Concretes to inſinuate themſelves into the pores of other bodies, and faſten their particles to themſelves, and to one another, either by wedging their Corpuſcles together, or by their ſtiff and ſlender parts, or their ſharp angles or edges piercing diverſe of them together; as when many Pieces of Paper are kept from ſcattering by a Wire that runs through them, or as when a Knife takes up at once diverſe pieces of Bread and Meat by being ſtuck into them all. But whenſoever there is in the conſtituent parts of the body a ſufficient fitneſs and diſpoſition to adhere firmly to one another, Nature may of thoſe parts compoſe a ſtable body, whether they abound in Salt or no, it not being ſo much upon Chymical Principles, or ev'n upon the Predominancy or Plenty of any one Ingredient, as upon the ſhape and motion of the component parts of bodies, that their Fluidity and Firmneſs depend. I will not here urge that Salts are generally reducible by an eaſie mixture with water into the form of Liquors; nor

that Sea-ſalt, Salt of Tartar, and diverſe other ſorts of Salts, will of themſelves, ev'n in the Air, if not very dry, aſſume the form of fluid Bodies; nor yet will I preſs the ſhortly to be mention'd Example of Coral, which is confidently affirmed to be ſoft whilſt it remains in the Salt water, and to grow hard when taken out of it. I will not here, I ſay, preſs theſe and the like Arguments, but content my ſelf to have hinted them, becauſe they are ſuch as I cannot well in few words make out and vindicate. Wherefore I ſhall rather demand, what Salt can be made appear to paſs out of the body of melted Lead into that of Quickſilver, to perform in it the coagulation abovemention'd? What acceſſion of Salt is there to be obſerv'd, when running Mercury is precipitated *per ſe* into a powder? and how will it be prov'd, that when in a well-ſtop'd glaſs the whole body of water is in froſty nights turned into firm Ice by the cold of the ambient air, that coagulation is perform'd by Salt, it having not yet been made appear by Chymiſts, that either Salts or even the diſtill'd Spirits of them can penetrate, without a kind of Prodigy, the narrow pores of unheated glaſs? It is uſually obſerv'd in Eggs, that though at their firſt coming out of the Hens belly, the ſhells are ſoft, yet ſoon after they grow hard and brittle; and yet it appears not how the ſaline Ingredient is encreas'd to effect this ſpeedy induration: and (to ſubjoyn that by the by) albeit I am not averſe from thinking that the coldneſs of the outward Air, and its imbibing ſome of the looſeſt of the moiſt parts of the ſoft Egge-ſhell, may concur to this effect; yet there are many Obſervations of Egge-ſhells that have been found hard in the womb of the Hen. And I well remember I have taken notice, that diverſe Eggs not yet laid, but found at one time in the body of the ſame Hen, were each of them furniſh'd with a compleat and brittle Shell. But I think I can draw a much ſtronger Argument againſt the Chymical opinion from

from the consideration of an Egg: for I demand what plenty of Salt can be made appear to pierce the hard shell, and more close-wrought membrane that both lines it and involves the Egg, especially since 'tis certain, that in *Egypt* and diverse other places Eggs may be hatch'd by a temperate external heat without the Hen. And yet we may here observe, that the same internal substance of the Egg which at first was fluid, the yolk and white that compos'd it being so, is upon the exclusion of the Chick turn'd almost all of it into consistent Bodies, some of them tough, as the membranes and gristles of the Bird, and some of them harder and almost brittle, as his bones and beak; and all this as we said without accession of new Salt. It would be hard for Chymists to prove, that Diamonds and Rubies, which are counted the hardest Bodies we know, (and at particular tryals of whose hardness I have sometimes wonder'd) do abound in Salt; at least it will not be unreasonable for us to think so, till Chymists have taught us intelligible and practicable wayes of separating (at least some) true Salt from either of those Jewels. And it may be also doubted whether the blood of Animals when it is freest from Serum, do not (though a Liquor) as much abound with Salt as their skins or their flesh.

And since 'tis with Chymists that I am now Reasoning, I presume I may be allow'd to press them with Arguments drawn from some of the Eminentest Writers of their Sect. For the generality of Chymists, and even those that are by the rest, and themselves too, call'd Philosophers, not only granting, but asserting and maintaining the Transmutation of great quantities of Quick-silver and the other ignobler metals into Silver or Gold by means of the white or red Elixir, I shall demand of them whence it happens, that one grain of the powder of Projection can turn a whole pound of Mercury into true Gold or Silver, and consequently change a very fluid Body into a very firm one, though the

pro-

proportion of Salt employ'd to coagulate the whole Maſs of Quick-ſilver would not amount to the ſix thouſandth or ſeven thouſandth part of the Liquor; though we ſhould grant that the powder employ'd to work this marvellous change were all of it Salt, to which yet Chymical Writers ſeem to aſcribe much more of the ſulphurous Nature. And to this I ſhall adde, what the famous and acute *Helmont* does to another purpoſe relate upon the Experience of *Raymund Lully* and his own, concerning his prodigious Liquor, Alkaheſt; namely, that being abſtracted from common Quick-ſilver, it does in a quarter of an hour coagulate it: and yet in this coagulation he points at this as a ſingular Phænomenon, that this Liquor which is as well immortal as exceeding ſaline, leaves nothing of it ſelf with the Mercury on which it works, and yet ſo coagulates it, that he preſcribes the making it into a ſubtil powder.

Helmont de Febr. cap. 14.

I remember alſo to our preſent purpoſe, that a Phyſitian of much veracity in what he relates, diſcourſing with me the other day about an odde preparation that he ſaw at the preſent Duke of *Holſtein*'s, (that Learned Prince and great Chymiſt) aſſur'd me that among other things he there took notice of a glaſs of Spirit of Urine, which in warm weather remain'd in the form of a Liquor, but in cold weather did totally coagulate into Cryſtalline ſalt: and being aſk'd by me if he knew how this Urinous body had been prepar'd? he anſwer'd me, that the Duke caus'd Spirit of Urine exceeding rich in volatile Salt to be diſtill'd very many times; after every Diſtillation re-conjoyning all that came over in a Liquid with that which remain'd in a ſaline form, till by very frequent cohobations all the parts of the Urinous ſubſtance were brought to the union or coalition above-mention'd. What we may propoſe concerning the various conſiſtence of the ſaline part of Urine upon our own knowledge, we ſhall for certain Reaſons reſerve for another place.

And

And on this occaſion we will annex a few particulars, which may tend not only to the making of the Chymical Hypotheſis about the coagulation of bodies doubtful, but to the confirmation of much of the Doctrine by us propos'd. The firſt ſhall be an Obſervation afforded us by the Art of making Sugar, wherein very great care is taken, that nothing acid (and eſpecially juice of Limons) fall into the Caldrons or other Veſſels wherein the juice of the Sugar-Cane is to coagulate into Sugar: for though acidity be generally by the Chymiſts aſcrib'd to Salt, yet here the ſaline bodies are ſo far from promoting the coagulation of the ſaccharine ſirrup, that they would quite hinder it. And becauſe that through the want of Sugar-Canes in theſe parts, we are reduced to take this Obſervation upon the credit of others, and becauſe alſo in it ſelf it ſeems ſomewhat ſtrange, we will vouch for it two eminent Authors in whoſe Writings we met with it. The one is the ingenious French Publiſher of the natural and moral Hiſtory of thoſe *American* Iſlands, commonly call'd by the French *Les Iſles Antilles*, and by us the *Caribe* Iſlands, who deſcribing particularly how his Country-men make Sugar in thoſe parts, gives this caution towards the latter end; *Sur tout*, &c. that is, Above all, great heed muſt be taken to let no juice of Citrons (or Limons) fall into the Caldrons, for that would abſolutely hinder the formation of the Sugar. The other is the diligent *Gulielmus Piſo*, who having given us a particular account both by words and pictures of the way of making Sugar, tells us that, *Si momentum ſucci Limonis vel acidi quid injiciatur, ſacchari conſiſtentiam nunquam acquiret, ſed in totum perditur.* To which I ſhall adde, that having purpoſely inquir'd concerning this Obſervation, it has been confirmed unto me by Perſons that pretend more than ordinary knowledge of the Art of ordering Sugar: which likewiſe affords us another Obſervation

Hiſt. Moral. Cap. 5.

Hiſto. Nat. & Med. Braſil. l. 4. c. 1.

not

proportion of Salt employ'd to coagulate the whole Mass of Quick-silver would not amount to the six thousandth or seven thousandth part of the Liquor; though we should grant that the powder employ'd to work this marvellous change were all of it Salt, to which yet Chymical Writers seem to ascribe much more of the sulphurous Nature. And to this I shall adde, what the famous and acute *Helmont* does to another purpose relate upon the Experience of *Raymund Lully* and his own, concerning his prodigious Liquor, Alkahest; namely, that being abstracted from common Quick-silver, it does in a quarter of an hour coagulate it: and yet in this coagulation he points at this as a singular Phænomenon, that this Liquor which is as well immortal as exceeding saline, leaves nothing of it self with the Mercury on which it works, and yet so coagulates it, that he prescribes the making it into a subtil powder.

Helmont de Febr. cap. 14.

I remember also to our present purpose, that a Physitian of much veracity in what he relates, discoursing with me the other day about an odde preparation that he saw at the present Duke of *Holstein*'s, (that Learned Prince and great Chymist) assur'd me that among other things he there took notice of a glass of Spirit of Urine, which in warm weather remain'd in the form of a Liquor, but in cold weather did totally coagulate into Crystalline salt: and being ask'd by me if he knew how this Urinous body had been prepar'd? he answer'd me, that the Duke caus'd Spirit of Urine exceeding rich in volatile Salt to be distill'd very many times; after every Distillation re-conjoyning all that came over in a Liquid with that which remain'd in a saline form, till by very frequent cohobations all the parts of the Urinous substance were brought to the union or coalition above-mention'd. What we may propose concerning the various consistence of the saline part of Urine upon our own knowledge, we shall for certain Reasons reserve for another place.

And

And on this occaſion we will annex a few particulars, which may tend not only to the making of the Chymical Hypotheſis about the coagulation of bodies doubtful, but to the confirmation of much of the Doctrine by us propos'd. The firſt ſhall be an Obſervation afforded us by the Art of making Sugar, wherein very great care is taken, that nothing acid (and eſpecially juice of Limons) fall into the Caldrons or other Veſſels wherein the juice of the Sugar-Cane is to coagulate into Sugar: for though acidity be generally by the Chymiſts aſcrib'd to Salt, yet here the ſaline bodies are ſo far from promoting the coagulation of the ſaccharine ſirrup, that they would quite hinder it. And becauſe that through the want of Sugar-Canes in theſe parts, we are reduced to take this Obſervation upon the credit of others, and becauſe alſo in it ſelf it ſeems ſomewhat ſtrange, we will vouch for it two eminent Authors in whoſe Writings we met with it. The one is the ingenious French Publiſher of the natural and moral Hiſtory of thoſe *American* Iſlands, commonly call'd by the French *Les Iſles Antilles*, and by us the *Caribe* Iſlands, who deſcribing particularly how his Country-men make Sugar in thoſe parts, gives this caution towards the latter end; *Sur tout*, &c. that is, Above all, great heed muſt be taken to let no juice of Citrons (or Limons) fall into the Caldrons, for that would abſolutely hinder the formation of the Sugar. The other is the diligent *Gulielmus Piſo*, who having given us a particular account both by words and pictures of the way of making Sugar, tells us that, *Si momentum ſucci Limonis vel acidi quid injiciatur, ſacchari conſiſtentiam nunquam acquiret, ſed in totum perditur.* To which I ſhall adde, that having purpoſely inquir'd concerning this Obſervation, it has been confirmed unto me by Perſons that pretend more than ordinary knowledge of the Art of ordering Sugar: which likewiſe affords us another Obſervation

Hiſt. Moral. Cap. 5.

Hiſto. Nat. & Med. Braſil. l. 4. c. 1.

not

not impertinent to the Theme we treat of; for the beſt Authors that write hereof inform us, that the juice ſqueez'd out of the Sugar-Canes is wont firſt to be boild and depurated in vaſt Veſſels of Copper or Braſs, whence it afterwards is convey'd to be further purifi'd and coagulated into ſmaller ones; and that whilſt it is in the former, they uſe to pour upon it ſome very ſtrong Lee to facilitate the ſeparation of its feculencies, as in the ſmaller ones 'tis uſual to pour a little Oyl or Butter upon the boyling juice, to keep the ſirrup from boyling over. Now that which they further obſerve to our purpoſe, is related almoſt after the ſame manner both by our French Author and by *Piſo*, and by the latter of them in theſe words, *Obſervatu dignum* (ſayes he) *ſi oleum majoribus inderetur ahenis in quibus Liquor primus, Caldo dictus, purificatur, ſaccharo conficiendo plane foret ineptus: viciſſim ſi minoribus lixivium ſicut majoribus infundatur, æquè impoſſibile ſaccharum conficere.* So much the Fluidity and Firmneſs of bodies depend upon their texture, how much ſoever Chymiſts would have them depend upon Salt.

Gulielm. Piſo, & Author Gallic. ubi ſupra.

But to this borrow'd Obſervation, though borrow'd of Authors not to be diſtruſted, we will adde two or three Experiments of our own, which we hope may the more confirm the Doctrine by us propos'd touching Stability in Bodies, becauſe it was our aim in them to bring light by them to the matters we treat of.

Firſt then, we prepar'd a Liquor elſewhere to be deſcrib'd, which is almoſt if not altogether as ſaline as *Aqua fortis* it ſelf, or any other acid Spirit that is commonly known: and yet when in this Liquor we laid fragments of ſolid Harts-horn of ſeveral ſizes to ſteep, ev'n in a cold place, the *Menſtruum* was ſo far from hardning them, that it would (without diſſolving them as corroſive Liquors do metals) gently pierce into them and ſoften them, ſo that

in

in about two or three dayes it would reduce them to a kind of white ſlime or mucilaginous ſubſtance at the bottom of the Liquor. We took alſo good ſalt of Tartar, and on it pour'd good Spirit of Vineger, as long as the affuſion of it would produce any ebullition: Then we diſtill'd off the Liquor, which came over almoſt inſipid, the ſaline parts that make Spirit of Vineger ſo ſharp, being retain'd by the Salt of Tartar: Upon the remaining dry mixture we pour'd freſh Spirit of Vineger as long as any hiſſing enſu'd thereupon, and afterwards abſtracted the aqueous parts of this parcel of Liquor alſo, and ſo we proceeded, till having ſufficiently impregnated the fix'd Salt with the ſaline parts of the diſtill'd Vineger, we obtain'd according to our deſire a mixture which (though it were all made up of Salts, and ſuch Salts too as being made by the Chymical Analyſis of the Bodies whence they were drawn, may according to the Chymical Doctrine be look'd upon as pure and Elementary) was yet ſo neer Fluidity, that it requir'd not the heat of the fire to turn it preſently into a Liquor, which ſhape it aſſum'd with a gentler warmth than one would expect from a ſaline Body. Laſtly, we took common Oyl of Vitriol, and caſt into it diverſe little pieces of Camphire, which floating upon it were by degrees and after ſome hours wholly reduc'd into a reddiſh Oyl, that was to be ſeen altogether upon the top of the other Liquor. Then having formerly try'd that Oyl of Vitriol would eaſily mix with common Oyl, we try'd alſo by ſhaking the ſaline and Camphorate Liquors together to unite them, and eaſily confounded them into one high-colour'd Liquor, which ſeem'd very uniform, and continu'd ſo (at leaſt as to ſenſe) for many hours. Then we added to this mixture three or four times as much fair water, and (as we expected) the Camphire immediately recover'd a white conſiſtent Body, and by degrees ſetled at the top of the Liquor: where we may obſerve, that the Camphire is not made hard but fluid

by its mixture with the ſaline Corpuſcles of Oyl of Vitriol, and exchanges its Fluidity for Firmneſs upon the affuſion of Saltleſs water. And thus much it may ſuffice to have ſaid touching the Chymiſts deriving the ſtability of Bodies from their abounding in Salt.

And as for the hardneſs and brittleneſs they aſcribe to the ſame principle, how much they may be increas'd or diminiſhed in a body without the acceſſion or decrement of the ſaline principle or ingredient, may appear by that Experiment mention'd by us to ſeveral purpoſes, of tempering a ſlender piece of Steel; for when it has been ſufficiently heated, by plunging it red hot into fair water, which is more likely to diſſolve than increaſe its Salt, you may make it not very hard alone but very brittle, whereas by only ſuffering it to cool leiſurely in the air, it will be both much leſs hard and more tough, and if after having quench'd it in cold water you again heat it till it have attain'd a deep blew, it will become (comparatively) ſoft and very flexible, and that not from any waſting of the ſaline ingredient by the fire, for if this ſoftn'd ſteel be again heated red hot and ſuddenly refrigerated, whether in water or otherwiſe, as before, it will re-acquire both hardneſs and brittleneſs.

Now that by theſe operations a real change is made in the diſpoſition of the ſmall parts of the ſteel, we have elſewhere evinc'd ev'n by a ſenſible proof. And that by procuring a cloſer order & more immediate contact of the parts of a body, a man may without encreaſing the Salt encreaſe the hardneſs of it, is, as we formerly alſo noted, obvious in Snow, which whilſt it lies in flakes as it falls upon the ground, compoſes but a ſoft and yielding body: But when the ſame ſnow is by being ſtrongly preſs'd every way betwixt the hands formed into Balls, the little whether Icecicles or frozen bubbles it conſiſts of, are ſo approach'd to one another, and forced into an order which allows ſo little waſt

waſt of room, that the formerly-intercepted ſpaces being moſt of them fill'd up with little bodies, the Iceicles can no longer yield as they did before to the preſſure of a mans fingers, but conſtitute a maſs conſiderably hard, which yet may be made harder being melted into water, and afterwards frozen into Ice; for this having been a fluid Body, (and in ſuch, Room is wont to be better husbanded than in others) the bubbles intercepted in it cannot keep it from being of ſo cloſe a texture as to be conſiderably hard.

I know that not only profeſt Chymiſts, but other perſons who are deſervedly rank'd amongſt the modern Philoſophers, do with much confidence entirely aſcribe the induration, and eſpecially the Lapideſcence of bodies to a certain ſecret internal principle, by ſome of them call'd a form, and by others a petrifying Spirit, lurking for the moſt part in ſome liquid vehicle. And for my part, having had the opportunity to be in a place where I could in a dry mould and a very elevated piece of ground cauſe to be digg'd out ſeveral Cryſtalline bodies, whoſe ſmooth ſides and Angles were as exquiſitely figur'd as if they had been wrought by a ſkilful Artiſt at cutting of pretious Stones, and having alſo had the opportunity to conſider divers other exactly or regularly ſhap'd Stones and other Minerals, ſome digg'd out of the Earth by my friends, and ſome yet growing upon ſtones newly torn from the Rock, I am very forward to grant, that (as I elſewhere intimate) it is a plaſtick Principle implanted by the moſt wiſe Creator in certain parcels of matter, that does produce in ſuch concretions as well the hard conſiſtence as the determinate figure. We deny not then, that theſe effects depend moſt commonly upon an internal principle, but the difficulty conſiſts in conceiving how that internal principle produces its effects, which theſe Writers not pretending to explicate intelligibly, we thought it not amiſs briefly to ſurvey ſome of the

principal ways by which it ſeems that Nature makes bodies firm and ſtable, whereby we may be aſſiſted to judge whether it be as neceſſary to have recourſe to a plaſtick Principle or a Gorgonick ſpirit in all other quick and notable Indurations of Bodies in the cold, as in the hardning of ſuch Bodies whoſe curious and determinate either internal Textures or outward ſhapes (common to ſeveral Concretions of one kind) argue their having been fram'd by ſome one formative power, or by diverſe ſeminal Principles conven'd together. But this we will do without affirming either that ſhe cannot by ſome other yet unobſerv'd way make conſiſtent bodies, or that of the ways by us diſcours'd of, ſhe is wont ſo to confine her ſelf to any one, that ſhe does not frequently make uſe of two or more of them to produce the ſame effect.

And becauſe Hardneſs is a high degree of Firmneſs, I ſuppoſe it will not be impertinent to ſhew by ſome examples how ſmall an external operation may without any appearing adventitious Salt make a ſoft body hard, and even brittle, when there appears not any other change to be made than that of the Texture or diſpoſition of its component particles.

It is a Tradition amongſt Naturaliſts, that Coral grows ſoft at the bottom of the Sea, but when it is brought up into the open Air, though it retains its bulk and figure, it hardens into a ſtony Concretion, according to that of *Ovid.*

Ovid. 15. *Metamorph.* *Sic & coralium quo primum contigit auras*
Tempore dureſcit, mollis fuit herba ſub undis.

Whether or no this Tradition is ſtrictly true, we had not opportunity when we ſtaid at *Marſeilles* (whoſe neighbouring Sea is the chiefeſt in *Europe* where Coral is wont to be fiſh'd) to give our ſelves an ocular ſatisfaction. But whatever

ever ſome ſay to diſcredit the tradition, nay, how confidently ſoever *Beguinus* (who ſeems to have the moſt ſtrongly argu'd againſt it) hath rejected it, it muſt not be denyed to be, ſometimes at leaſt true, (and that's enough to ſerve our preſent turn.) For the Learned *Gaſſendus* in the Life of *Piereskius*, relating how that incomparable Gentleman had the curioſity to fiſh for Coral near *Toulon*, (a noble Port not far from *Marſeilles*) has among other things this paſſage, (*viz.*) The plants which were pluckt up and drawn out were neither red nor handſome till their Bark was pull'd off; in ſome parts they were ſoft, and would give way to the hand, as towards the tops, which being broken and ſqueez'd they ſent forth milk, like that of Figs. I remember likewiſe, that the Learned Jeſuite *Fournier*, who being alſo a French Hydrographer, and one that writes of *Marſeilles* and *Toulon* as places very well known unto him, may be ſafely credited on this occaſion, after he has particularly deſcrib'd the way of fiſhing Corals near *Toulon*, he adds, Theſe plants are neither red nor poliſh'd when they are drawn out of the water, till their Rind have been taken off, nay, they are ſoft, and being broken or elſe ſqueez'd betwixt the fingers, they throw out a kind of milk reſembling that of Figs; and when one leaves off preſſing them, he may ſee the ſmall holes or pores that harbour'd the milk that was ſqueez'd out. Thus far He. The credibleneſs of a good part of theſe narratives has been confirmed to me by a practiſer of Phyſick in the Eaſt-Indies, who having made ſome ſtay at his return on the Iſland of *Mehila*, (near that of *Madagaſcar*) where ſtore of white Coral is reported to grow, I enquired of him whether he had gathered any, and whether he found it ſoft whilſt it was growing? and he anſwer'd me, that he had

Beguin. Tyrocin. Chym. Lib. 2do Cap. 10.

Lib. 4°. *Anno Domini*, 1624.

Hydrograph. de P. Fournier. Lib. 4. Cap. 27.

had of late years diverſe times gather'd Coral upon the Sands of that Iſland, and found it, when he gather'd it, exceeding white, and (to uſe his expreſſion) as ſoft as an Onion, adding, that though it would in a very ſhort time grow hard in the air, (which he aſcrib'd, how juſtly I know not, to the external heat of the Sun) yet it is very well known to the Sea-men that deal in that ware, that if it be not gather'd at a ſeaſonable time of the the year it will not keep long, but either crumble away or otherwiſe decay, which diſagrees not with the experienc'd *Piſo*, who in his natural Hiſtory of *Braſil*, ſpeaking of ſome places of the *Braſilian* Coaſt, where diverſe ſtony plants, ſome like little Trees, ſome otherwiſe fram'd, may be ſeen in clear weather growing in the bottom of the Sea, tells us, that, *è fundo erutæ mox duriſſimæ, ſi inſolentur in littore, ſiccæ niveique coloris fiunt.* As remarkable a change is that I meet with in *Scaliger*, who tells us as upon his own knowledge of ſome, who at the Urinary paſſages voided a ſlimy matter, which in the Air coagulated into a firm ſubſtance; the ſtory being memorable, take it in his own words thus: *Ex bovillis oppidanus noſtris adjutus medicamentis eminxit vitrum ſane ex illa nobili Paxagoræ pituita, dum mingeretur albuminis mollitie emiſſum vitri duritie ac ſplendore, Senatoris filius ejecit, pultis modo multos, & maximos: qui aeris contactu poſtea in gypſeam tum ſpeciem tum firmitatem concrevere; hic quoque nunc rectè valet.* Having likewiſe had the acquaintance of an inquiſitive Merchant of *Dantzik*, and alſo of an ingenious Chymiſt, that ſpent ſome time in that City and the neighbouring Country, along whoſe coaſt our European Amber is wont to be dragg'd out of the Sea, I enquir'd of them, whether they had never obſerv'd in Amber a property like that which is reported of Coral: and one of them, as I remember the other alſo, hath aſſured me upon his own particular Obſervation, that lumps of

Lib. 4. *Cap.* 68.

of Amber are ſometimes taken ſoft out of the Sea, and grow hard in the Air; which is the more credible to me, becauſe I have at a *Polonian* noble-mans ſeen (beſides other intercepted things) a whole Spider, and that none of the leaſt, perfectly incloſed in a piece of hard and tranſparent yellow Amber. And elſewhere I have ſeen ten or twelve (if I miſ-remember not the number) pieces of ſuch Amber, which contain'd, one a Fly; another a Spider, a third a Straw, and each of the reſt ſome ſuch other thing. And it ſeems not impoſſible, that the contract of the external air may put the parts of ſuch ſmall Bodies into a new motion, whereby ſome voluble Corpuſcles that hinder their reciprocal adheſion may be excluded, and the particles themſelves preſt or otherwiſe diſpos'd into a cloſer order; and we find that ſome Oyl-colours, after they are brought to their due temper, may be preſerv'd very long in the ſame degree of ſoftneſs, if they and the ſhells that contain them be kept all the while under water, whereas in the air they would quickly change their Texture, and become dry and hard.

But though in this laſt mention'd Example, and ſome others the removal of the body out of the water into the air ſeem manifeſtly to contribute to its growing hard, yet it ſeems not to us ſo eaſie to determine what ſhare the air has in effecting ſuch indurations: for *Gaſſendus* relates of *Piereskius*, that he being wont in the Summer time to waſh himſelf in one of the leſſer ſtreams of the River of *Rhoſne*, he there made the following Obſervation. Once upon a time he felt the ground, which he had wont to find even and ſoft, to be grown hard with little round balls or bunches, like hard boiled Eggs when the ſhell is peel'd off; at which wondring, he took ſome of them up, and carried them home, that he might ſhew them to his Maſter, and demand of him the Reaſon. But the miracle was increas'd when a few

Gaſſend. in vita Piereskii, lib. 1.

few days after returning to the River, he found thoſe little balls or lumps turned into perfect pebble ſtones, which he obſerv'd likewiſe to befal thoſe which he had carried and laid up at home. But how far this ſtory will prove that ſuch coagulations muſt be effected by a ſubſtantial form or a petrifying Liquor, we define not, eſpecially ſince, not to repeat what we deliver'd already touching calcin'd Marble out of *Fournier*, we have elſewhere deliver'd upon our own Obſervation, that two or three ſpoonfuls of ſuch pap of burnt Alabaſter as we have lately been ſpeaking of, (and inſtead of which Artificers uſe another ſtone call'd by them Plaſter of *Paris*, burnt and and temper'd up with fair water) did in the bottom of a veſſel-full of water into which we pour'd it in a ſhort time coagulate into a hard lump, notwithſtanding the water that ſurrounded it; which, it ſeems by the Texture of the maſs, was kept out of its pores, as it is out of thoſe of the Oyls of Cinnamon and Cloves, which though fluid bodies, and ſinking in water, ſuffer not its particles to inſinuate themſelves into theirs: and Artificers obſerve, that the coagulating property of burnt Alabaſter will be very much impair'd, if not loſt, if the powder be kept too long, eſpecially in the open Air, before it be made uſe of; and when it has been once temper'd with water and ſuffer'd to grow hard, they tell me they cannot by any burning or powdering of it again make it near ſo ſerviceable for their purpoſe as before; ſo much doth the coagulation of theſe powders mixt with water ſeem to depend upon their Texture and other Mechanical qualities.

I remember alſo, that though the bones found in the Hearts of Deer, and ſo magnified by Phyſicians, do in the air acquire a hard and bony conſiſtence; yet having had the curioſity to conſider one of them in the Heart of a Deer newly kill'd, I found it there of a cartilaginous ſoftneſs and flexibility.

And

And here I will adventure further to confeſs, that I have oftentimes doubted whether or no not only conſiſtent Bodies but ſome of the moſt ſolid ones in the World may not have been fluid in the form either of Steams or Liquors, before their coalition and their concretion either into ſtones or other mineral Bodies. I know there are many who think that Stones, Marchaſites, and other ſuch ſolid and durable Bodies, were made together at the Creation or other beginning of the Univerſe, and who will not admit that ſuch concretions can be now generated. But not here to debate that famous Controverſie, whether ſtones may be ſaid to grow and to be nouriſh'd, in the ſtrict ſenſe of thoſe Expreſſions, I think it not difficult to ſhew that ſuch parcels of matter are now to be met with in the form of ſtones as did not before appear under that form, but whilſt it was divided into minute parts whether was it ſelf ſome fluid Body or other, or at leaſt as a material part concur to the conſtituting of that was ſo. Of this, beſides what we elſewhere deliver concerning it, we ſhall anon have occaſion to mention ſome proofs; and therefore we ſhall now only mention two or three inſtances. The firſt whereof ſhall be, that we ſaw, among the Rarities of a Perſon exceedingly curious of them, a ſtone flat on the outſides, on one of whoſe internal Surfaces was moſt lively engraven the Figure of a ſmall Fiſh, with all the Finns, Scales, *&c.* which was affirmed to have been encloſed in the Body of that ſtone, and to have been accidentally diſcover'd, when the ſtone chancing to receive a rude knock upon its edge, ſplit aſunder. I remember alſo that a while ſince a Houſe-keeper of mine in the Country informed me, that whilſt a little before he caus'd in my abſence one of my Walls to be repair'd, the Maſon I was wont to employ caſually breaking a ſtone to make uſe of it about the Building, found in it (to his wonder) a piece of Wood that

ſeem'd part of the branch of ſome Tree, and conſequently was afterwards enclos'd with that ſolid caſe wherein he found it. This cavity in the body of the ſtone and, as I remember, the ſtick it ſelf he took out of it, he forthwith brought my Houſe-keeper, to whom I have given directions to ſend them me. For this example ſeems to me a more cogent proof of the increaſe of ſtones, than ſome others that eminent Naturaliſts much rely on, for Reaſons diſcours'd of in another place: where we alſo make particular mention of that Ghur or Metalline juice, which though the Latin Writers of Chymical and ev'n of Metalline matters have not, that I remember, given us any account of, yet I find a German or two, that were very converſant in the Mines themſelves, to have in Books written in their own Language taken a ſpecial notice of it. Beſides, I have at preſent ſomething to deliver upon my own Obſervation, which unleſs we will ſuppoſe (what ſeems not probable) that there were from the beginning made together with and in the midſt of great Maſſes of one kind of Mineral little parcels of another of a quite differing ſort, ſeems manifeſtly enough to argue, that either whole quarries of ſtone, or heavy and ſhining Minerals, or both, may have been fluid Bodies. The Obſervation whereon I ground this Conjecture is, not only that we have met with in Lead-oar and alſo in *Minera Antimonii* parcels of a white ſtone or ſpar environ'd with a Metalline body, though I think I have yet by me ſuch lumps of Oar; but chiefly that I have with my own hands taken a hard and ponderous ſhining Mineral, which I keep for a Rarity, like a Marchaſite, of the ſhape of a Pear, and of about the bigneſs of a Walnut, out of the very Body of a ſtone wherein I ſuſpected it to be enclos'd, and which environ'd it on all ſides: and this I took not out of a ſmall and looſe ſtone, but a large ſtone digg'd out to make Sta-

In ſome of the Authors Papers about the Origine of Minerals.

tues

tues of. And I remember that one of thoſe that wrought upon it told me, that in faſhioning it into Satues they found ſome more Minerals in the ſame parcel of ſtone, which were alſo preſented me. To which I ſhall adde, that an ingenious Statuary having in another place taken much pains to ſaw aſunder a very large ſtone, when he came to the midſt of it, found he could ſaw on no farther, and the ſtone being afterwards broken, he perceiv'd that that which ſo obſtinately reſiſted his Saws was a round Marchaſite, which he brought to me, as a Lover of ſuch Curioſities. But I made him for my further ſatisfaction bring me alſo that part of the ſtone wherein the Marchaſite ſtuck, and by comparing them together diſcern'd that as much of the ſtone as was contiguous to the Marchaſite had a kind of ruſt about it, and fitted the Marchaſite ſo cloſe, as if either the Marchaſite had been formerly liquid, and had afterwards been as it were moulded in that Receptacle, or the ſtone had been formerly of ſome ſoft or fluid matter, which did exactly accommodate it ſelf to the ſhape of the other Body; or elſe, as if both the matter of the ſtone and that of the Marchaſite had been at once fluid Bodies, but had each of them preſerv'd its own ſurface diſtinct (according to what we formerly noted of differing fluids) till one of them (probably the Marchaſite) firſt growing hard, the other, as being yet of a more yielding conſiſtence, accommodated it ſelf to the harder's figure.

But the moſt eminent Inſtances to declare how much the Fluidity and Firmneſs of Bodies depend upon the contrivance and Texture of their parts, are afforded us by thoſe waters which being permitted to reſt a while do ſpontaneouſly ceaſe to be fluid and coagulate into ſtone it ſelf. There was lately an Ingenious Man, who going to viſit ſome Leaden Mines wherein he had a ſhare, found in the Mountain in whoſe Entrals they were hid a Cave, from whoſe arched Roof there drop'd down a petrefcent Liquor,

which oft-times before it could fall to the ground congeal'd, and by appoſition of like matter increas'd ſo much that they hung from the Roof like Ice-icles in a froſty night from the ſides of a Houſe; and of theſe he gather'd and brought me diverſe, which are perfect ſtones hard and brittle, and of eight or ten inches long, and proportionably thick. Another ingenious friend of mine being lately in *France* in the Cave ſo famous for petrifying Liquor to be there ſeen, obſerving ſome drops of water to congeal into ſtone whilſt he ſtood by, took them away with him, and ſent them me in a Letter. Nay, we ſhall ſcarce deny that an external agent of almoſt inſenſible bulk may turn animal Bodies into ſtony ones, by introducing a new texture into their parts, if we will with ſome modern Writers believe *Aventinus*, who in his *Bavarian* Hiſtory has recorded, that at a time and place by him ſpecified, above forty Country-men, as alſo ſome Milk-maids with their Cows kill'd upon an Earthquake, had their Bodies by a terrene Spirit turned into ſtatues, which he ſayes were ſeen by the Chancellour of *Auſtria* and himſelf. And ſome relations of this Nature we meet with in other Authors, which, if they be allowed of, ſeem much to confirm our Doctrine; for in theſe ſtrange petrifications, the hardning of the Bodies ſeems to be effected principally, if not only, as in the induration of the fluid ſubſtances of an Egg into a Chick, by altering the diſpoſition of their parts, ſince the petrifying wind or ſteam cannot be ſuppos'd to have any ſuch conſiderable (perhaps not any ſenſible) proportion as to bulk to the body chang'd by it, as to be thought to effect this change principally as an Ingredient.

Adde we to all theſe things, that *Pamphilio Piacentino* is by another Author quoted for writing an unparrel'd Story, which becauſe written in Italian, I ſhall Engliſh the ſubſtance of it, which is this: That a Woman in *Venice*, after having eaten an Apple, was taken with hideous tortures, and

and in the ſpace of twenty four hours dying, was turned into exceeding hard ſtone, and this was judged to be the effect of the poyſon'd Apple ſhe had eaten. Which narrative, if we may believe it as confidently as the famous Alleger of it *Pamphilio* appears to do, would ſeem to argue, that even to the wonderful induration of Bodies there is ſometimes no other principle requiſite than what may reſult from the lucky mixture of the parts of ſeveral Bodies. And leſt we ſhould ſeem to build altogether upon the Obſervations of others, which cannot by us be now brought to ſtrict examination, we will have recourſe to a practicable Experiment of our own trying, which though we have elſewhere mention'd, we ſhall not ſcruple here to repeat, becauſe we there omitted to ſpeak of that Circumſtance of it, which is the moſt pertinent to our preſent deſign.

Take then two Ounces of Quick-ſilver, two Ounces and a half of the beſt Verdigreeſe, about half an Ounce or an Ounce of common Salt, a pint or pound of White-wine-Vinegar, and as much fair water, mingle the Verdigreeſe, Quick-ſilver, and the Salt very well, and put the mixture with a little of the Vinegar and water into a new Frying-Pan, (I try'd it in a new Earthen Veſſel, but without good ſucceſs) in which fry it over the fire for diverſe hours, keeping it continually ſtir'd, and putting in more Vinegar and water from time to time, as that already put in conſumes away; then take out the mixture, and in ſeveral clean waters waſh it carefully from the adhering Salts; then dry away all the Aqueous moiſture with a clean linnen Cloth, and you ſhall have a bright Amalgama almoſt like Quick-ſilver. Now that which is remarkable and to our preſent purpoſe in this Experiment is, that though this dry'd mixture be a good while after it is perfectly cold not only ſoft, but ſo neer to fluid, that I have caſt it into moulds and made imboſt Images of it, (when it has been dexterouſly made, but ſcarce otherwiſe;) I have found, that by laying it a few

hours

which oft-times before it could fall to the ground congeal'd, and by appoſition of like matter increas'd ſo much that they hung from the Roof like Ice-icles in a froſty night from the ſides of a Houſe; and of theſe he gather'd and brought me diverſe, which are perfect ſtones hard and brittle, and of eight or ten inches long, and proportionably thick. Another ingenious friend of mine being lately in *France* in the Cave ſo famous for petrifying Liquor to be there ſeen, obſerving ſome drops of water to congeal into ſtone whilſt he ſtood by, took them away with him, and ſent them me in a Letter. Nay, we ſhall ſcarce deny that an external agent of almoſt inſenſible bulk may turn animal Bodies into ſtony ones, by introducing a new texture into their parts, if we will with ſome modern Writers believe *Aventinus*, who in his *Bavarian* Hiſtory has recorded, that at a time and place by him ſpecified, above forty Country-men, as alſo ſome Milk-maids with their Cows kill'd upon an Earth-quake, had their Bodies by a terrene Spirit turned into ſtatues, which he ſayes were ſeen by the Chancellour of *Auſtria* and himſelf. And ſome relations of this Nature we meet with in other Authors, which, if they be allowed of, ſeem much to confirm our Doctrine; for in theſe ſtrange petrifications, the hardning of the Bodies ſeems to be effected principally, if not only, as in the induration of the fluid ſubſtances of an Egg into a Chick, by altering the diſpoſition of their parts, ſince the petrifying wind or ſteam cannot be ſuppos'd to have any ſuch conſiderable (perhaps not any ſenſible) proportion as to bulk to the body chang'd by it, as to be thought to effect this change principally as an Ingredient.

Adde we to all theſe things, that *Pamphilio Piacentino* is by another Author quoted for writing an unparrel'd Story, which becauſe written in Italian, I ſhall Engliſh the ſubſtance of it, which is this: That a Woman in *Venice*, after having eaten an Apple, was taken with hideous tortures, and

and in the ſpace of twenty four hours dying, was turned into exceeding hard ſtone, and this was judged to be the effect of the poyſon'd Apple ſhe had eaten. Which narrative, if we may believe it as confidently as the famous Alleger of it *Pamphilio* appears to do, would ſeem to argue, that even to the wonderful induration of Bodies there is ſometimes no other principle requiſite than what may reſult from the lucky mixture of the parts of ſeveral Bodies. And leſt we ſhould ſeem to build altogether upon the Obſervations of others, which cannot by us be now brought to ſtrict examination, we will have recourſe to a practicable Experiment of our own trying, which though we have elſewhere mention'd, we ſhall not ſcruple here to repeat, becauſe we there omitted to ſpeak of that Circumſtance of it, which is the moſt pertinent to our preſent deſign.

Take then two Ounces of Quick-ſilver, two Ounces and a half of the beſt Verdigreeſe, about half an Ounce or an Ounce of common Salt, a pint or pound of White-wine-Vinegar, and as much fair water, mingle the Verdigreeſe, Quick-ſilver, and the Salt very well, and put the mixture with a little of the Vinegar and water into a new Frying-Pan, (I try'd it in a new Earthen Veſſel, but without good ſucceſs) in which fry it over the fire for diverſe hours, keeping it continually ſtir'd, and putting in more Vinegar and water from time to time, as that already put in conſumes away; then take out the mixture, and in ſeveral clean waters waſh it carefully from the adhering Salts; then dry away all the Aqueous moiſture with a clean linnen Cloth, and you ſhall have a bright Amalgama almoſt like Quick-ſilver. Now that which is remarkable and to our preſent purpoſe in this Experiment is, that though this dry'd mixture be a good while after it is perfectly cold not only ſoft, but ſo neer to fluid, that I have caſt it into moulds and made imboſt Images of it, (when it has been dexterouſly made, but ſcarce otherwiſe;) I have found, that by laying it a few

hours

hours in the air, which ſeem'd leſs cold than it ſelf, it has acquir'd ſuch a hardneſs, that being thrown againſt the floor it would rebound, and was brittle like over-harden'd Steel. And yet in this Example the induration of the Amalgam appears not to proceed from an innate and inward principle, but from the new Texture reſulting from the coalition of the mingled Ingredients that make up the Amalgam, whoſe parts being variouſly moved, partly by the fire (and perhaps too by the Salts) and partly by the native propenſity to motion of the Mercurial Corpuſcles, were by little and little, or by degrees, ſo diſpos'd, that whereas before touching one another but looſly, it was eaſie to thruſt ſome of them towards the middle of the body without ſtirring much of the Maſs (as to ſenſe) by this change of Texture the particles are brought to touch one another more cloſely and in greater portions of their ſurfaces, and to be ſo complicated, intangled, or otherwiſe connected among themſelves, that you cannot endeavour to thruſt one of them out of its place, but that its motion ſhall be reſiſted by many others, to whom it is ſo faſten'd, that you cannot move one part of the Maſs without either moving the whole with it, or manifeſtly breaking it off from the whole, and thereby deſtroying the continuity and unity of the Body.

Now whereas in ſetting down this Experiment, we ſpoke as if ſeveral Ingredients did concur to conſtitute the ſoft Maſs, which afterwards grew ſo hard, we might very ſafely do ſo, ſince the Quickſilver was not ſo barely chang'd in Texture as that formerly ſaid to have been coagulated by the meer fume of Lead, but conceal'd in its ſelf a great number of metalline Corpuſcles beſides others, as we made appear by ſeparating from the Amalgam, meerly by the force of fire, a pretty quantity of true and perfect Copper. That the Salts alſo both were Ingredients (though in ſmall proportion) of the Maſs, and might have ſome opera-

operation upon the other particles, we may render probable by this, that having purpoſely expos'd ſome of this Maſs for a pretty while to a moiſt Air, we found, as we look'd for, that the formerly inviſible particles of Salt, that had ſo inſinuated themſelves into the Amalgam, that all the water wherein it was waſh'd did not ſeparate them from it, had ſo wrought upon the metalline particles that were moſt outward, that they had in many parts of the ſurface of the Maſs turned themſelves with it into a kind of Verdigreeſe, which ſeemed almoſt to hide the ſurface of the Concretion. And that in the more inward parts of a much harder Body than our yielding Amalgam, where Cuprious particles abound, ſaline Corpuſcles may have a great operation, may appear by certain ſorts of Minerals to be found in ſome parts of *England* and elſewhere under the form of ſtones, of which they make Vitriol; for theſe abounding with vitriolate, that is, both ſaline and metalline particles, will, after they are taken out of the ground and laid in the open air, by the working of the inward Salt, ſome ſooner and ſome later, ſwell and burſt aſunder, which could hardly come to paſs without a great change made in the internal diſpoſition of the parts, of ſuch ſtony Concretions. And I remember, that having laid a mineral of Kin to theſe ſtones a while in the air, though but in a Chamber, I found its ſurface powder'd with little grains of Vitriol, as both their Colour and their Taſte inform'd me.

Now whether or no we ſuppoſe that the fire did put the parts of the Amalgama into any laſting Agitation, yet the Maſs being almoſt fluid after it was taken from the fire, its parts may according to our notion of Fluidity be well ſuppos'd to have ſome kind of motion among themſelves; and it will not be deni'd, that the fire might concur with other things to make that motion convenient to cauſe the parts to faſten themſelves to one another: For that the motion

wherein

wherein a ſoft and almoſt fluid Body is once put may poſſibly tend to harden it long after that motion ſeems to be extinct, may be made probable by what has been affirm'd to me by eminent and experienced Maſons, namely that the beſt ſort of Lime made into Mortar will not have attain'd its utmoſt compactneſs till twenty five or thirty years (perhaps not till three or fourſcore) after it has been imploy'd in Building; and this is given me as one of the Reaſons, why in the demoliſhing of antient Fabricks, it is ſometimes more eaſie to break the ſtone than the Mortar.

And laſtly, that we alſo made mention of the Texture reſulting from the mingled Ingredients of our Amalgam, we might juſtifie by ſaying, that having changed the proportion of the Quick-ſilver to the Verdigreeſe, we found that the Amalgam coagulated much more ſlowly, and when it was coagulated, was much leſs hard than when one uſed the quantities above ſpecify'd.

Here I ſhould put a period for the preſent to this Diſcourſe, but that having in a late Writer met with a notable Obſervation of the natural Induration of a ſoft Body, I think it worthy to be here annex'd, partly, becauſe the French Book is not common, no more than the Obſervation; and partly, that by conferring together this natural Induration with that Artificial one freſhly mention'd, it may the better appear how Nature and Art have ſometimes reſembling operations in rendring Bodies ſolid. My Author then (by name *Pierre Pelleprat*) being not long ſince ſent with ſome other Jeſuites upon the laudable errand of Preaching the Goſpel to the Indians of the Southern *America*, has among other things this paſſage in the ſhort Relation he makes of the American Continent. There is (ſays he) one thing worthy of Obſervation neer the mouth of this great river (he ſpeaks of that of the *Amazons*) which is,

Relation des Meſſions des P. P. J. J. ſecon. part, cap. 1.

is, That men find there a kind of green Clay that is ſoft as long as 'tis in the water, ſo that one may print on it all kind of Figures, and give it what ſhape one pleaſes; but when it is expos'd to the Air, it hardens to that degree that Diamonds are not much harder than the ſtones it affords ----- I have (adds he) ſeen Hatchets made of this Clay, which the Savages employ'd to cut Wood with, when they had not the uſe of ours, *&c.*

And now at laſt, I ſee 'tis time to put a period to a diſcourſe, that has been unawares lengthned far too much already: But yet I think you will eaſily pardon me, if I conclude it not abruptly; but with the recital of an Experiment, which having had the honour to be ſeen, as to the main part of it, by an illuſtrious meeting of Curious Men; their having been pleas'd to ſpeak very advantagiouſly of it to others, excited a curioſity among them, to know by what artifice, effects that were ſo uncommon, had been produc'd. The Scope therefore, and the manner of making the Experiment, were in ſhort as follows.

Being deſirous to ſhew how much Fluidity and Firmneſs may depend upon the Texture and upon the Motion or Reſt of the inſenſible parts of Bodies, I firſt make with good Spirit of Vinegar, a Solution of Coral ſo ſtrong, that when 'tis filtrated and cool'd, it will commonly, after ſome time, begin to have a kind of Sediment at the bottom; the clear Liquor I gently pour off, when the Experiment is to be made, and to this I put a convenient proportion of very well dephlegm'd Spirit of Wine, which if it be pour'd on very ſlowly and warily, may be made for a pretty while to ſwim upon it in the form of a diſtinct Liquor: but if by a few ſhakes I mingle them together, they will preſently unite into a Concretion, of which when the Experiment ſucceed very well (as it did when I ſhew'd it to the above-mention'd Aſſembly) not

one drop will fall to the ground, upon turning up the wide mouth Glaſs it ſhould be made in, and holding it with the mouth directly downwards.

And this ſo haſtily produc'd conſiſtence may be durable enough, if it be carefully lookt to: But to diſpatch the whole Experiment in a ſhort time, I take a little ſtrong Spirit of Nitre (which perhaps is not needful if good Aqua Fortis be at hand) and putting about an equal, or other convenient quantity of it to this Mixture, I nimbly ſtir it and the Spirit together: whereupon the Whole is reduc'd in a very few minutes to a tranſparent Liquor.

N. B. Though I have divers times made and ſhewn this Experiment, yet there are ſo many Circumſtances requiſite to make the firſt part of it ſucceed very well (for to make it ſucceed in ſome meaſure is not ſo difficult) that the event has ſometimes deceiv'd me, in ſpite of the ſeveral Tryals I have made. Wherefore 'twill not be amiſs to intimate.

Firſt, That one of the firſt times, if not the firſt, I made ſuch an odd Concretion, was, with the Solution not made with Spirit of Vinegar, but with Spirit of Verdigreaſe: (which I commonly diſtil without additament) though afterwards I was invited to prefer ſtrong Spirit of Vinegar, which was the Liquor wherewith the recited Experiment was exhibited.

Secondly, That it often happens that if the Solution of Coral (which is not the only body wherewith I have made ſuch Tryals with indifferent good ſucceſs) be not ſufficiently ſtrong and impregnated) with Saline parts, or the Spirit of Wine be not ſufficiently rectify'd, the ſhaking of the two Liquors will not change the conſiſtence of the whole mixture, but leaves ſome part of it fluid, or elſe the Concretion will not begin preſently to be made, but require to be waited for a while.

Thirdly, That I once at leaſt (if not oftner) obſerv'd, that

that when by mingling the two Liquors and ſhaking them in a narrow mouth'd Glaſs whoſe Orifice was ſtopt, they would not concoagulate (as it was confidently expected they ſhould) yet by trying the Experiment in a wide mouth Glaſs to which the Air had free acceſs, it ſucceeded to my content.

Fourthly, That in the Reduction of the Concretion to a fluid Body, 'tis not proper to employ in ſtirring it a Knife or any other Metalline Body except it be of Gold; but rather ſome Stick of Glaſs, or at leaſt ſome clean Stick of Wood, leſt the *Menſtruum* ſhould corrode it, and thereby ſpoil, or at leaſt blemiſh the Experiment.

Fifthly, That the proportion betwixt the Coralline Solution and the Spirit of Wine depends ſo much upon the ſtrength of the former Liquor, and the dephlegmedneſs of the latter, that 'tis ſcarce poſſible to determine generally and exactly what quantity of each ought to be taken; and therefore a Tryal or two made with a little of the particular Solution you intend to employ (for ſome Solutions require more, others leſs Spirit of Wine to concoagulate adequately with them) will better direct you what proportion of Spirit will ſute that particular parcel of Liquor then any general Rule I can propoſe.

I know I might here, and perhaps it may be expected that I ſhould, take an occaſion to treat alſo of Hardneſs, Softneſs, Brittleneſs, Toughneſs, Stiffneſs, and thoſe other qualities that are of kin to Fluidity and Firmneſs; but though I confeſs, I once had thoughts of writing a kind of Hiſtory of more Qualities than thoſe, yet remembring that wiſe Counſel given us by one of the Antients, *Noſcenda eſt menſura ſui*, I am now very well content, after having already tir'd my ſelf, and I fear you, to recommend ſo uſeful but difficult a work to Perſons more able and more at leiſure than I find my ſelf to go through with ſo great an undertaking; contenting my ſelf at preſent, to have attempted

in what has been delivered concerning a couple of qualities of ſuch extent, that every ſenſibly big Body in the Univerſe ſeems indow'd with one or other of them (I mean concerning Fluidity and Firmneſs) the explicating of Qualities ſomewhat more intelligibly than is wont to be done in the Peripatetick Schools, and to have open'd a way (which I hope many will tread) of applying Chymical Obſervations and Experiments to the deduction of thoſe effects of Qualities from ſuch general and obvious affections of matter; as Bigneſs, Motion, and Figure, which even the Hermetical Writers have hitherto contented themſelves to refer to Salt, Sulphur, Mercury, and the like: the Chymical notion of which (three Principles) though of very good uſe in ſome other (eſpecially of the more practical) parts of Phyſiology, ſeems not as yet to have brought any great light to ſuch matters as we have been treating of, having been hitherto directed not ſo much to the indagation of Cauſes, as to the production of Effects.

The End of the Notes touching Fluidity *and* Firmneſs.

FINIS.

OF

Abſolute Reſt

IN

BODIES.

LONDON,

Printed for *Henry Herringman* at the *Blew Anchor* in the Lower Walk of the *New-Exchange*, M DC LXIX.

AN

ADVERTISEMENT.

SInce it hath not been thought amiſs that ſomething ſhould be intimated to the Reader *about the Occaſion of the enſuing Tract, I ſhall acquaint him with it as briefly as I can, by telling him that it was This. Some very ingenious Gentlemen hapning to meet as Viſitants at the Author's Lodgings, fell accidentally into a Diſcourſe about the* Abſolute Reſt *ſuppoſed to be in many* Bodies, *that ſeem'd to have its Riſe from a miſtake of the true meaning of a paſſage or two in the* Hiſtory of Fluidity and Firmneſs, *(that was then re-printing.) But the Conference chancing to have a period put to it, whilſt ſeveral things pertinent to the Author's purpoſe remained yet unſaid: the Curiouſneſs of the Subject invited him to draw up (haſtily enough) the Sum of what he had ſaid, and might further have ſaid if opportunity had ſerv'd, about the Point in debate, for the* further *ſatisfaction of an inquiſitive* Virtuoſo *that was preſent at it. And this was the Riſe of the following Diſcourſe, which being written on an Occaſion adminiſtred by the* Hiſtory of Fluidity and Firmneſs, *whereof a New Edition was ready to come abroad; 'twas thought not improper that this Tract ſhould attend it, as a kind of Appendix, without the Firſt and Laſt part of a Letter, whereof the Body only is neceſſary to the Deſign of it.*

AN

AN

ESSAY

Of the

INTESTINE MOTIONS

Of the

PARTICLES

OF

QUIESCENT SOLIDS.

Where the Abſolute Reſt of Bodies is called in Queſtion.

SECT. I.

TO remove the Doubt or Scruple that began to be diſcourſed of juſt before we laſt parted, I ſhall need to do little more than enlarge the Particulars, which (you know) I had then time but briefly to make mention of. For the ſtate of the Queſtion was, (as you may remember) this, *Whether there be among Bodies*

any abſolute Reſt? On which occaſion I anſwered, That Reſt being a word that to me ſeem'd ſomewhat ambiguous, I thought 'twas requiſite to clear the ſenſe of the Queſtion before I offer'd at anſwering it.

For the word Reſt, when we ſpeak of diſtinct Maſſes of Matter, lookt upon as quieſcent, does in the vulgar acception of the Term ſignifie, ſuch a ſtate of a viſible and entire Body, or (rather) of the Corpuſcles it conſiſts of, that they are actually *Unmoved as to ſight*; the Eye (and perhaps not the Touch) being not able to diſcern any local Motion in them.

Conſonantly to this firſt Member of the diſtinction of the word *Reſt*, I briefly intimated to the Company, that in this ſenſe of a Corpuſcles being at Reſt, I thought it manifeſt, that there is ſuch a thing in *Rerum Natura*: Since without granting ſuch a Reſt in the component Particles of ſome kind of Bodies, as Diamonds, Iron, Porphiry, *&c.* 'twill be (I conceive) very hard to explain, how there can be ſuch ſolid Maſſes (as thoſe Minerals are) made up of ſmall and ſeparable Particles. Which being ſaid, I added, that I ſaw no reaſon why ſuch a kind of firmneſs, where the inward motion of the inſenſible Particles is almoſt infinitely ſlow, may not ſuffice to give an account of as great a firmneſs as we uſe really to find among conſiſtent Bodies.

But whereas I had intimated to the Company, by the lately begun Diſtinction, that beſides this popular ſenſe of the word Reſt, there was a ſecond, more rigid and Philoſophical Notion, or kind of Reſt, which for diſtinction ſake may be called Abſolute or Perfect Reſt; which imports a continuance of a Body in the ſame place *preciſely*, and includes an abſolute Negation of all local Motion, though never ſo ſlow or imperceptible; I told them that in this rigid ſenſe of the word Reſt, I durſt not affirm, that there are any Bodies at Reſt in the Uni-

Univerſe (at leaſt for any long time) but willingly allowed it to be made a Problem, whether there be any or no: adding, that perhaps I enclin'd to the Negative part of the Queſtion.

Having thus hiſtorically ſumm'd up what paſs'd betwixt us about the ſtate of the Controverſie, I need not tell you, that the Doubt I expreſs'd was thought to reliſh too much of a Paradox; and therefore ſince the company's quick ſeparation allow'd me then no opportunity of enlarging, and ſince I promis'd no better Arguments than might be expected in a point that I propos'd but as Problematical; I ſhall now endeavour to ſhew you that the ſide of the Problem I was judg'd enclin'd to, is (at leaſt) not ſo improbable as ſome thought it.

To prove Negatives directly, being wont (as you well know) to be no eaſie Task, and eſpecially in ſuch caſes as this; you will not, I preſume, expect that I ſhould attempt the proving of my Conjecture otherwiſe than by ſhewing poſitively, that ſome of thoſe Bodies which we think to have their parts moſt at Reſt, are not exempted from having Inteſtine Motions in them; ſince 'twill be conſequent to ſuch a proof, that it muſt be probable, that in other Bodies whoſe Solidity is confeſſed to be inferiour, the component Particles are not in a ſtate of Perfect Reſt.

SECT. II.

If it were neceſſary and expedient, I ſhould begin my Arguments by ſaying ſomething againſt Abſolute Reſt, in favour of the contrary Opinion, by arguing, *à Priori*, as they ſpeak, from the conſtitution of the World, whether we conſider it according to the Epicurean or the Carteſian Hypotheſis of the Origine of things. For the Epicureans ſuppoſing this World to be produced

by the casual concourse of Atoms, and ascribing to every particular Atom an innate, and unlooseable mobility, or rather, an actual motion, or a restless endeavour after it; 'tis consonant to think, that though in Concretions, they so entangle one another, that they cannot in a short time, or a visible manner clear themselves from one another, yet they do incessantly strive to dis-entangle themselves and get away: by which means there is always in the Atoms even of Solid Bodies, actual endeavours of each of the distinct Atoms to extricate it self from the rest: (which endeavours usually at last succeed, whence comes the decay and destruction of Bodies) and in the mean while these perpetual and contrary endeavours produce intestine Commotions in the internal parts of the Body wherein these Atoms were imprison'd.

On the other side according to the Cartesian Doctrine, the *Materia Subtilis*, that constantly passes like a stream through the Pores even of the solidest Bodies, may well be suppos'd in its passage to be continually shaking or otherwise agitating the insensible Particles that make up the body that seems to be at Rest, without discovering their Motion to the Eye: As when in Summer time (to explain my self by a Comparison) a gentle breath of Wind passes through a Grove removed a pretty way off from the Spectator, though his Eye discern no change in the Grove he looks on, yet the Wind as it blows through the Trees will shake some of the Branches as well as the flexible Twigs; and not only blow the Leaves into various postures, but blow some of them quite away.

I might easily enlarge on this Subject, but having elsewhere done it on another occasion, I think it may be now more proper to satisfie some of the Company, who are yet entangled with the same prejudice with many other

other very Learned men, who look upon it as a Precarious and Chimærical Fancy of the Atomiſts, to imagine, that in Solid, and as to ſenſe, Quieſcent Bodies, there ſhould be any inteſtine Motion of the component Particles, neither the Motions nor the Corpuſcles themſelves being to be ſeen, and both of them being therefore as well incredible as inviſible.

A ſolemn Debate of the whole Queſtion about the Minuteneſs of Atoms belongs not to this place, where it may ſuffice to anſwer the Objection.

SECT. III.

And firſt, As I have elſewhere hinted, it may appear by divers of the Phænomena above-cited (in the *Hiſtory of Fluidity*) that when Water and ſeveral other Liquors ſeem to be continued Maſſes of Matter, and to be as much at reſt, as the very Glaſſes that contain them; their conſtituent Corpuſcles are in an actual and various though ſlow and unperceived Motion.

Next, That there may be likewiſe ſuch a Motion in the minute parts of Silver and Iron themſelves, may be eaſily argued, by heating thoſe Metals till they come to be almoſt red hot: for then though the eye can diſcern no motion of the Corpuſcles thoſe Metals conſiſt of, yet their being able to burn thoſe that hold them in their naked hands, ſhews that their brisk Motions may be diſcovered by the help of the Touch; and if you ſpit upon them the Liquor will boil, as if it were over the fire. And leſt it ſhould be objected, that ſo anomalous and violent an Agent as the Fire, is neceſſary to theſe Tryals, I ſhall add, that, provided the minute parts be ſufficiently agitated, it matters not whether the Motion be produced by Fire or no; for by the nimbly hammering of Iron or Silver, you may put the minute parts into ſuch a Motion, as will make the Metal very hot

to

to the Touch, and being communicated to Spittle or Water, will excite Bubbles, and ſcatter the diſſipated parts of that Liquor into the Air, in the form of Smoke or Vapours; nay I elſewhere ſhew how I have eaſily excited a very ſenſible, though not a viſible agitation, and heat in the internal parts of a Metal, barely by my naked hands, without any external inſtrument whatſoever.

And whereas it may be objected, that though the Motion already generated is unſeen, yet we may diſcern a change of the component Corpuſcles of a Body which are in the Act of altering its Texture, and introducing a new alteration or quality in the Body to be wrought on, or deſtroying ſome pre-exiſtent quality: I briefly anſwer (for I would not here repeat what I have elſewhere ſaid of this point) by this clear Experiment, that though your Eye can diſcern no change in the outward and viſible, much leſs in the more latent and internal Corpuſcles of Iron: a vigorous Loadſtone by paſſing along its Axis from one Pole of the Stone to the other, and back again, yet the Texture of the Iron is by that action of the Load-ſtone ſo changed, that it acquires, and then loſes thoſe admirable Qualities we call the Attractive and Directive virtue or faculty peculiar to Magnetical Bodies.

And to ſhew you that the inviſible Motions even of Metalline Bodies may be quick and briſk enough, and may be ſenſible, though not viſible; We ſhall need to conſider but the ſtate of a good Bell ſo long after the Clapper has ſtruck it, that no ſhaking or other Motion is to be ſeen in the body of the Bell it ſelf, and yet it cauſes in the Air an odd kind of ringing, or if I may ſo call it, undulating Sound or Motion, which will ſometimes laſt a conſiderable while; and if the Bell be fitted for ſharp notes, 'twill not be without a ſhrilneſs: for if

ſounds

ſounds proceed, as is elſewhere made probable, from the nimble percuſſions of the Air put into a quick and waving motion by ſonorous Bodies; this acuteneſs of ſound will ſhew, that whilſt to the Eye the Bell ſeems to be at Reſt, yet the minute parts of it continue in a very brisk motion, without which they could not ſtrike the air ſtrongly and faſt enough to make it produce ſo ſhrill a noiſe in the Ear.

But, I confeſs to you, that my thoughts preſent me a Difficulty, which though un-mention'd at our meeting, may afford an Objection, perhaps more difficult than any of (not to ſay all) the foregoing, namely, That 'tis ſcarce imaginable, how ſuch ſolid and hard Bodies ſhould have their internal parts wrought upon by ſuch ſlight Agents as the air, and perhaps ſome yet minuter matter that is diſperſed in it; and how it is poſſible, that where there is an actual Motion it ſhould be ſo ſlow, that a Corpuſcle of Iron, for inſtance, ſeated in the internal Part of a Magnetick Needle, ſhould ſpend ſo long time as our conjecture requires in travelling ſo little a ſpace as from thence to the next Superficies of the Needle. But to this double Objection, though ſome inſtances which you will meet with in the following part of this Paper, may be properly applyed to ſolve it: yet not to make your curioſity wait, I will here ſpeak a word or two to each of the members of the Objection.

SECT. IV.

And to the firſt, I ſay, That theſe Inteſtine Motions of the Corpuſcles of hard Bodies, need not be ſolely, nor perhaps principally aſcribed to thoſe obvious external Agents, to which we are wont to refer them, ſince theſe may but excite or aſſiſt the more principal or internal Cauſes of the Motions we ſpeak of, as you may gather

ther from what was but lately mention'd of the connate and unlooſeable mobility of the Atoms, according to *Epicurus*, and the permeation of the moſt Solid Bodies by the Carteſian *Materia Subtilis*; and we may ſee by the ſudden effects of the Load-ſtone, in endowing Steel with Magnetick Qualities, and depriving it of them again (both which ſuppoſe the intervention of a change of Texture, and this a production of Local Motion in the Metal) that very minute and inſenſible Corpuſcles of matter are not uncapable of effecting durable changes in the ſolideſt Bodies.

And as for the other member of the Objection, I confeſs it is not eaſie for us who are wont (perhaps too much) to follow our Eyes for Guides in judging of things corporeal, and to deny exiſtence to moſt things, to moſt things whereto Nature has deny'd a viſible bulk: 'tis not eaſie, I ſay, for us to imagine ſo great a ſlowneſs as 'tis very poſſible for Nature to make uſe of in her Operations, though our not being able to diſcern the motion of a ſhadow of a Dial-plate, or that of the Index upon a Clock or Watch ought to make us ſenſible of the incompetency of our eyes to diſcern ſome motions of natural Bodies, which reaſon tells us ought to be incomparably ſlower than theſe. But not now to diſpute about the exiſtence and Attributes of infinite ſlowneſs, or at leaſt a ſlowneſs in the next poſſible degree to infinite: I conſider that it has not that I know of been demonſtrated, nor attempted to be ſo, that the motion of the Corpuſcle, for example of the Needle above mention'd, muſt be made in a direct line from the place where 'twas firſt ſuppoſed to be to the Superficies of the Needle; for it ſeems more rational, and to agree better with the Phænomena, to ſuppoſe, that the way of this Corpuſcle in the Body 'twould quit, is extreamly crooked and intricate (almoſt like that of a Squib in the air, or on the ground) for it being on the one hand

hand urg'd on by the Cauſes whatever they be that make it ſtrive to fly away, and on the other hand hindred by the Corpuſcles whereto 'tis connected, and by the occurſions of other Corpuſcles whoſe motions may be oppoſite to, or diſagreeing with thoſe of our deſign'd Corpuſcle; it may probably, before it can extricate it ſelf, be reduc'd to encounter and wreſtle, as it were, with many other Corpuſcles, and be by them ſometimes thruſt or impell'd to the right hand and to the left, and ſometimes alſo repell'd inwards, even after it is come to the ſuperficial part of the Needle; whence it may not preſently have the liberty to fly away, but may be drawn back by ſome other Corpuſcle, wherewith it is yet connected, and which happening to be it ſelf thruſt inward may draw after it, and ſo entangle again our almoſt diſbanded Corpuſcle: beſides that, the gravity of the component Particles of a Body is oftentimes ſuch, that 'tis eaſier for the Agent that puts them in motion, to continue them in that ſlow motion among themſelves, than drive them up into ſo light a *medium* as the air, as experience ſhews in thoſe Bodies that are called Fixt, as Gold, and Glaſs, though in actual fuſion.

But, I forget that I promis'd you to decline Speculations, and therefore I ſhall only name to you a couple of Inſtances which will ſerve to confirm both what I was lately ſaying, and what I am now in proving.

SECT. V.

The firſt of theſe I ſhall take from what is uſually granted as matter of Fact, namely, that if a Spring, though made of ſo hard a Body as Steel, be forcibly bent, and kept but a moderate while in that poſture, as ſoon as the force that kept it bent is removed, it will again return to its former Figure; but if it be kept too

long in that forc't position, it will by degrees lose that which they call the motion of Restitution, and retain its new crooked Figure, though the force that bent it be removed; which shews both the power of some of the more familiar Agents in Nature, and (which is that the shewing whereof I here chiefly aim at) that where there is a continued endeavour of the parts of a Body, to put themselves into another state, yet the motion, or rather the progress may be much more slow than men seem as yet to have taken notice of, since 'twas a great while before the Texture of the Corpuscles of the Steel were so alter'd as to make them lose their former springiness.

But I will second this with a more illustrious Experiment, which will at once confirm what I have just now said, and shew that the Air or the invisible Corpuscles harbour'd in it may have no inconsiderable power to act upon, and effect changes in the solidest Bodies.

To this purpose I shall only observe to you, that *though* if a Bar of Iron having one of its ends held perpendicularly, and at a fit distance, to the Lilly or North-Point of the Mariners Compass (I mean that which points towards the North) it will, as I elsewhere mention, drive it away towards the East or West: and if this same lower end of the Bar of Iron be put into a contrary posture, it will presently lose its temporary magnetism, as I elsewhere declare. Yet if this Bar be very long kept upright in a Window or other convenient place, then, as some late Magnetical Writers will tell you, it will have acquired a constant and durable magnetick power. Which is a Phænomenon that makes exceedingly for our present purpose, since it hence appears both that the Air together with the magnetical Effluvia of the Earth that it receives in its Pores, is able without outward force to work durable changes in so

solid

ſolid a Body as Iron, and that the motions of the internal Parts, for theſe are requiſite to the change of the Metal's Texture, are performed with a wonderful ſlowneſs, ſince the Bar muſt be very long expoſed to the air, perhaps before it acquires any durable magnetiſm at all, but at leaſt before it acquires ſo vigorous and fixt a magnetiſm as by this means it may attain to.

But, becauſe moſt of the Inſtances to be propos'd in the following part of this Eſſay, may ſerve for Confirmations of what we have been diſcourſing; I ſhall proceed to them, yet not 'till I have advertis'd you, That I purpoſely decline to mention divers Phænomena that may even by Learned men be thought fit examples on this occaſion, (ſuch as the Nutrition, Growth, and Waſting of Animals and Vegetables) becauſe ſuch Bodies receiving conſtant ſupplies of Corpuſcles, of ſeveral, and often unknown, Natures, there may be difficulties ſuggeſted about them, not eaſie to be cleared without longer Diſcourſes than I can allow this Eſſay.

SECT. VI.

The firſt Inſtance then that I ſhall mention about Vegetable ſubſtances, ſhall be taken from *Lignum Vitæ*, or *Guaiacum* (for many Artificers give them the ſame name, and uſe them promiſcuouſly for the ſame purpoſes;) of which, though it ſeem to be the ſolideſt wood we know (inſomuch that I as wel as ſome others have ordinarily us'd it to pound ſolid Bodies in) yet the skilfulleſt Tradeſmen I have met with, have upon my inquiry informed me, that if it be wrought before it be well ſeaſon'd by length of time, it will ſhew it ſelf very frangible; which an eminent Turner told me he had often found to his loſs: For having turn'd divers fine pieces of Work of *Lignum Vitæ*, before 'twas duly ſeaſon'd, he found almoſt all

of them by the heat of the Sun (which the uſes of many of them requir'd they ſhould ſometimes be expos'd to) crack, and cleave aſunder, into I know not how many parts; whereby thoſe fine pieces of workmanſhip were quite ſpoil'd. And I remember, that having enquir'd of an old experienced Tradeſman, of whom I bought an excellent Mortar of *Lignum Vitæ*, how long he had kept the Wood in ſeaſoning before I had the Mortar, he anſwered me (if I much miſ-remember not) 20 years, under which time it is not fully ſeaſon'd for ſome purpoſes; of which opinion of his, having occaſionally ſpoken to the lately mention'd Turner, this experienced Workman much confirmed me in it; as he likewiſe did in an Obſervation I not long ſince made about the ſlow and unperceived motion that may be, not only in the more looſe and fugitive Aqueous parts of *Lignum Vitæ*, but in far more unlikely ones. For he told me that he had often found, in Turning that Wood, Cavities of ſeveral ſizes in the very inward and ſolid part of the Wood (which every way encompaſs'd them) and in thoſe Cavities conſiderable quantities of a certain Gum, much cryed up by ſome for an Anti-Venereal Medicine.

The uſe I would make of theſe Examples is this, That ſince ſo ſolid a Body as a Trunk of *Lignum Vitæ* is, when the Tree is newly fell'd, may require ſo long a time as 20 years, or upwards, to be ſeaſoned (*i.e.*) brought to its full compactneſs and toughneſs; and ſi[illegible]he account upon which time ſeaſons Wood ſeems to be this, That by degrees the looſer Aqueous, and more fugitive parts exhale into the air, whereby the remaining ſolid ones are brought into a cloſer order, and have leiſure to be ſo placed among themſelves, as is moſt convenient to make their Texture firm and durable: it will follow, that even in the internal parts of this ſolid Wood there muſt be, not only in the looſer and lighter Corpuſcles, that extri-

extricate themſelves, and exhale away, a true local motion, though much too ſlow to fall immediately under the diſcernment of our ſenſes. And, if the lately mentioned Gum be either totally, or ſo much as in part generated, as to ſenſe, after the felling of the Tree, as ſome Analogous Inſtances that I have elſewhere taken notice of, make it probable; then the Example will further be conſiderable to our preſent purpoſe, by ſhewing that a ſubſtance ſo groſs, and ſo little volatile in compariſon of the Aqueous parts, as is the brittle Gum I ſpeak of, may permeate to a great thickneſs, a very ſolid and inanimate ſubſtance; which cannot be done without an inteſtine, though inſenſible motion among the parts of the Wood, and probably a marvellouſly ſlow motion of thoſe of the Gum.

SECT. VII.

But it will poſſibly ſeem more ſtrange, that very thin pieces of Wood, and thoſe ſaw'd off from a Tree of a much looſer Texture, ſhould be much longer in ſeaſoning than that ſolid and ponderous Wood we have been ſpeaking of. And indeed this diſcovery is not to be made, as in *Lignum Vitæ*, by the brittleneſs, or other obvious qualities in the Wood, but by a ſubtler way; and accordingly having purpoſely conſulted with the Makers of Muſical Inſtruments, and with ſome ancient Muſitians, I was much confirmed by them in my opinion: And I remember, the laſt Maker of Viols, Lutes, *&c.* of whom I enquired of what Age he thought ſuch Inſtruments, eſpecially Lutes, ought to be to attain their full and beſt ſeaſoning for ſonorouſneſs; he reply'd, that in ſome of them 20 years would be requiſite, in others 40, according to the nature and thickneſs of the Wood, and other circumſtances. But an Ancient Muſician that was preſent

preſent at what was ſaid, inform'd me that there were ſome famous Lutes, one or two of which he nam'd to me, that attained not their full ſeaſoning and beſt reſonance till they were about fourſcore year old. And thus much for inanimate Vegetable ſubſtances.

SECT. VIII.

As for calcin'd Stones made up into Lime, and ſorts of raw ſtones, I have already obſerved from the credible relations of Maſons and others, that the Walls in ſome Buildings attain not their hardneſs and ſolidity till they are 40 years old, or perhaps much ancienter; and ſince in gradually proceeding to this degree of ſolidity, theſe Walls reſemble the ſeaſoning of *Lignum Vitæ* formerly explicated, the motion of the internal parts may be argued from the change of Texture as well in theſe as in that.

And, if I would rob other Tracts (to which they more properly belong) I could here eaſily adde ſome ſuch Inſtances of the hardening and ſoftning of Stones by time, as would much confirm what I have now been delivering; but I ſhall rather chuſe to confine my ſelf here to the two Examples following, not taken notice of in Quarrys or by Maſons.

The firſt is, That there are Marchaſites, conſiſting as well of a ſtony as of a metalline ſubſtance, which, though harder than many other ſorts of Stones, and even than Marble, have yet ſo great a motion in their internal parts, that if they be expos'd to the air, not only they will have a Vitriolate Efflorescence, if I may ſo ſpeak, on their ſuperficies (as I have obſerv'd in divers other Marchaſites) but they will in Tract of time burſt the Stone in Pieces; of which ſort I had ſome time ſince, and I hope I have yet a bulky Marchaſite that I procur'd from

from a Virtuoso that lives just by a Vitriol-work, whither these among other Vitriol-Stones are brought, and where this Stone being chosen for its largeness, was taken up and carefully kept by that ingenious person till it burst of it self, and till I sent for it. And to satisfie my self a little further, that the internal parts of Marchasites may be as well dispos'd to be vitriolated as the external; I remember I broke a hard Marchasite that I had from another place, and laying it some short time in a Chamber-Window, I found the new superficies made by the Tracture about the middle of the Stone to have acquired an Efflorescence of a vitriolate Nature.

The other instance, which is very odd, and much talked of, is this: An ingenious Gentleman of my acquaintance, casually meeting me one day, told me that he had a Turquois-stone, which if he were not mistaken had a wonderful property, for there being in it several spots of Colours differing from the rest of the Gem, these spots seem'd, though very slowly, to move from one part of the stone to the other. And this he thought himself to have taken notice of for very many Months (perhaps a couple of years). This Relation seem'd so strange that the Relator was not at all surpriz'd, when to ascertain my self of the truth of it, I desired to have the Ring this stone was set in, for a while in my own keeping, to which he readily assenting; besides that I took very heedful notice of the scituation of the spots, I employ'd a very ingenious youth that then lived with me, and was skill'd in drawing, to make the Picture of the stone with the spots as they were then placed, and afterwards to have a watchful eye upon it, and from time to time (as once perhaps in two or three weeks) to draw a new Picture of them; by comparing several of which Pictures, it was unanimously concluded that the spots did shift places in the Turquois, as if the matter they consisted of made

made its way through the ſubſtance of the ſtone: As we lately noted that the Gum of *Lignum Vitæ* ſeem'd to do through the ſubſtance of the Wood: And as far as we obſerv'd, the motion of theſe ſpots was exceeding ſlow and irregular, though perhaps it might have been reduc'd to a ſomewhat leſs uncertainty, were it not that by an unwelcome accident we were deprived of the opportunity of continuing our Obſervations long enough. And this brings into my mind, that the Turquois being a ſtone, of which I had met ſtrange ſtories in good Authors, I once asked ſeveral queſtions about it, of a noted Jeweller; and enquiring among other particulars, whether he had not obſerv'd ſome changes that ſeem'd ſpontaneous in the ſubſtance or colour of the ſtone? he reply'd that in ſome few Turquois's he had obſerv'd two differing Blews in differing parts of the ſame ſtone, and that one of thoſe Colours would by ſlow and unperceived degrees invade, and at length overſpread that part of the ſtone, which the other Colour poſſeſſed before. I ſhall here add, that the ſame Gentleman that lent me the ſpotted Turquois, ſhew'd me afterward an Agate Haft of a Knife, where was a certain Cloud, which he told me an ingenious Perſon had for ſome years obſerv'd to remove to and fro in the ſtone, and had a while ſince to convince the Relator lent him the Agate, of whoſe Phænomena he promis'd me an account, when he ſhall have had the ſtone in his cuſtody for a competent time, till the expiration of which, it may ſuffice to have ſaid of this Agate what I have now related.

SECT. IX.

But becauſe that Diamonds and Glaſs are generally looked upon, eſpecially by Chymiſts, as Bodies of the cloſeſt and firmeſt Texture that Nature and Art afford, if

if we could ſhew an inteſtine motion even in the parts of theſe; fitter Inſtances for our purpoſe could not in reaſon be deſired: I ſhall venture to ſay ſomething of each of them, though what I have to ſay about Diamonds, is propos'd rather to ground a ſuſpition of what may be, than a demonſtration that it muſt be.

In the firſt place then, to remove that prejudice that may be entertain'd from the incomparable hardneſs of Diamonds (which I confeſs experience has made me admire) as if Bodies ſo hard and ſolid could not have their parts put into motion but by ſome extraordinary, not to ſay, prodigious force; I ſhall only repeat here what I have elſewhere ſhewn, that Diamonds are Bodies that eaſily enough become actually Electrical, and that ſome Diamonds (of which ſort I have a ſmall one by me) will by rubbing upon a cloath be brought to ſhine in the dark, the Quiſt of both which tranſient Qualities requiring a change of Texture even in the internal parts; and the Friction that produces that change, doing it immediately by putting the parts of the ſtone into local-motion, it may be thence argued, that a very moderate force may ſuffice to beget an internal motion in the inward Particles of Diamonds themſelves.

And I am not ſure but that more hidden Agents may make impreſſions upon theſe hardeſt Bodies. For in a Ring that I am wont to wear on my little Finger, which has no Diamond, ſave one more than that ſhining one I lately mention'd, I have I know not how often ſeem'd to my ſelf to obſerve a manifeſtly greater clearneſs and ſparklingneſs at ſome times than at others, though I could not refer it to the clearneſs or dulneſs of the weather, the moiſture or drineſs of the air, the ſuperficial clearneſs or foulneſs of the ſtone, or any other manifeſt

cauſe I could think of. And in this I was the more enclin'd to think I might not be miſtaken, becauſe, beſides that the notice I took of it, was frequent, I have by me a rough Diamond juſt as it came from the Rock, in whoſe Electrical faculty I have taken notice of changes as to the degree of ſtrength wherewith it attracted, and that within the compaſs of a very little time, though I could not find any cauſe whereto I could refer ſo ſurprizing a Phænomenon. And I muſt not here omit, that chancing one day to ſhew the newly mention'd Diamond Ring to a very ingenious Lady that uſed to wear in Rings and Bracelets ſtore of thoſe Gems, and telling her what changes I had taken notice of in the Diamonds; ſhe who had obſerv'd more about Gems than any Lady I had yet met with, appeared but little ſurprized at what I told her, and affirm'd to me that ſhe had divers times obſerved the like alterations in ſome Diamonds of hers, which ſometimes would look more ſparklingly than they were wont, and ſometimes far more dull than ordinary. And when I objected, that poſſibly that dulneſs might be imputed to the weather, or ſome caſual foulneſs of the ſurface of the ſtone, ſhe reply'd that ſhe had been aware of thoſe circumſtances, rubbing the ſtones clean, and otherwiſe taking care to ſecure an Obſervation, which ſhe had made too often to have deceiv'd her ſelf in it. If I remember aright, I have elſewhere mention'd how I ſaw a conſiderable, but Cloudy, *Hungarian* Diamond, which the Owner would have preſented me, made clearer by lying for ſome time in a cold Liquor, wherein he affirmed that upon his keeping it longer the ſtone would loſe more and more of its cloudineſs; and what I my ſelf ſaw ſuffic'd me to argue, that changes may be produc'd even in the inward parts of ſuch Diamonds by Agents that act without any appearing Violence.

SECT.

SECT. X.

And if it be true that Diamonds, as I elſewhere obſerve about many other ſtones, may be generated from time to time in the bowels of the Earth, it may not perhaps be abſurd to imagine, that even true Diamonds, that ſeem perfect, and are fit for Rings, may long continue to have an inſenſible motion through the whole ſtone, whereby the Corpuſcles it conſiſts of are diſpos'd into a more convenient Texture for the conſtituting of an extreamly hard body. For though it be taken for granted, that Adamantine Bodies, becauſe they are generally exceeding hard, are equally ſo, yet that ſuppoſition is not favoured by Experience. And I remember, that to ſatisfie my ſelf further about ſuch matters, I repair'd to an ancient Artificer eminent in his Trade, which was the cutting and ſetting of Diamonds, and that having demanded of him whether he found that all Diamonds were of equal hardneſs; he anſwered me, that having dealt in theſe Gems near 20 years in *Amſterdam*, and divers years in *England*, he perceived that there are of later years brought over worſe and worſe ſorts of Diamonds, and that he finds ſeveral of the recent Diamonds ſo ſoft and brittle in compariſon of thoſe he was anciently wont to ſet (and which he with other Jewellers called Diamonds of the old Rock) that he is often afraid, and unwilling to meddle with them, when they are brought to him, leſt he ſhould ſpoil them in the cutting, or poliſhing. But this I only repeat hiſtorically, till further obſervation ſhall diſcover whether theſe are Diamonds not yet fully ripe, and capable of growing harder by further maturation, or whether they be of a peculiar ſort of Diamonds whoſe nature it is to be always ſofter than thoſe of the old Rock.

SECT. XI.

This brings into my mind a confirmation of the unequal hardneſs of Diamonds, whatever be the cauſe of it, which I met with in a little Book lately Publiſhed in his own Language by a Frenchman, who giving his Reader an Account of the Eaſtern Diamond-Mines from the Relation, as he affirms, of a late Eye-witneſs, ſpeaks thus of the third and laſt Mine called *Gazerpoli*, *They are very clear, and of a good Water, but they cannot be ground by mutual Attrition* (if I underſtand the Term he uſes) *but with ſtones of the ſame Mine; for if one ſhould employ for that purpoſe the ſtones of another Mine, thoſe of* Gazerpoli *would be broken in pieces: They do alſo eaſily break upon the Wheel, and thoſe that are not vers'd in the knowledge of ſtones may eaſily be deceived* (in them.) Of which our Author addes the Example of a *Portuguais*, who refuſing 1200 Crowns for one of them at *Ligorn*, when he went to have it cut at *Venice*, it broke upon the Wheel into fifteen or twenty pieces.

Egrezes, pag. m. 17.18.

Another Example that ſeems to make more for our preſent purpoſe is afforded by the ſame Author, ſpeaking of the ſecond Mine, which breeds the greateſt ſtones called Gane or Colonor; for he ſays, that *ſur la plus part*, *i. e.* upon the moſt of theſe ſtones after they are cut, there appears always as it were a kind of greaſineſs or unctuoſity, which invites you ever and anon to have recourſe to your Handkerchief to wipe it off, which one would gueſs to proceed from ſome inſenſible Effluvium, that exhaling out of the ſtone comes to be check'd and condens'd by the air on the ſuperficies of it, as it happens to ſweat on the skins of Animals: the truth of which conjecture I would examine by very nice ſcales, if I could procure ſuch Diamonds.

SECT.

SECT. XII.

And becauſe Rubies, though inferior in hardneſs to Diamonds, are yet harder than moſt other Gems, and much more than Marble and the like courſer ſtones, I will not omit on this occaſion, what was more than once affirmed to me by an obſerving Lady, whom, if ſhe were not too nearly related to me, I could ſcarce mention without an Elogy. For caſually caſting my eye upon a fair Ruby ſhe wore upon her finger, and deſiring to conſider it more attentively, ſhe pull'd off the rich Ring 'twas ſet in, and reaching it me, told me 'twas worth my curioſity to conſider it. For beſides that 'twas ſo fine a ſtone, that 'twas thought worth being left her as a Legacy by a great Lady (her dear friend) that was famous, as I knew, for the variety of the rich Jewels ſhe was Miſtreſs of; this Ruby would not unfrequently vary the degrees of its luſtre ſhe knew not why. For ſometimes it ſeem'd to be ennobled by a more vivid fire than ordinary, and at other times it would be manifeſtly more dull and cloudy than 'tis wont to be: and this not imputable, as ſhe expreſly aſſur'd me upon repeated Obſervations, to the Cloudineſs of the Weath r, or any ſuperficial foulneſs of the ſtone. And that I might be convinc'd as well as ſhe her ſelf was, ſhe deſir'd me to rub it very clean, and then take notice of the preſent luſtre of it, of which e're long ſhe preſum'd ſhe could ſhew me a manifeſt alteration (for I was then come to viſit her and paſs ſome weeks with her in her houſe) but my occaſions calling me away within a few days after, I had not time to wait for the event of her promiſe.

How far what has been ſaid concerning Diamonds may be allowed to be Argumentative towards the ſcope of this Diſcourſe, I ſhall willingly leave to the diſcovery

of

of time, and further Obſervation; the mention I have made of the foregoing particulars, having been invited partly by the nobleneſs of the ſubject, which made me willing to adde theſe Relations to what I have elſewhere written about them; and partly becauſe thus much at leaſt ſeems probably deducible from what I noted about the exciting of Diamonds by rubbing, both to attract, and to ſhine, that notwithſtanding their incomparable hardneſs, an inteſtine motion of their minute parts may be without any conſiderable violence quickly produc'd.

SECT. XIII.

And now 'twill be time to conſider the other Body I promis'd to take notice of, namely Glaſs. For this being thought ſo compact and firm a Body, that 'tis indiſtructible by Art or Nature, and being alſo of ſo cloſe a Texture, that the ſubtleſt Chymical Spirits (that are yet known) cannot pervade it, and laſtly having given ſuch proofs of the fixedneſs of its parts, as to have long endur'd the violence even of a Glaſs-houſe-fire, we can ſcarce imagine a Body more unlikely to have any motion amongſt its component Particles: and yet that they may not be always at perfect Reſt among themſelves, I have been induced to think by the following, and the like Obſervations.

Firſt, having enquired of a famous and experienced Maker of Teleſcopes, as well as of thoſe that uſe ſuch Inſtruments, whether he did not obſerve that the Venice-Glaſſes he employed would ſometimes crack of themſelves whilſt they were yet in Plates, and ſometimes do the like after they were ground into Convexes, and poliſhed up; he anſwered affirmatively. And though it ſeem'd improbable that Glaſſes brought ſo far

far off as from *Venice*, and many of them kept a good while here in *England* before there be occaſion to grind them: and perhaps longer after their having been ground before they crack, ſhould after all this time retain an internal motion among their component Particles: yet I have been induc'd to conjecture that ſome ſaline Corpuſcles more numerous than the Nature of the Glaſs requir'd, may, by degrees though ſlow and unperceiv'd, ſo tend towards the ſuperficies of the Glaſs, as either to get out of the Pores of it, or crack, or burſt the Glaſs in endeavouring to force their paſſage outward. For having purpoſely enquired of the abovenam'd Artificer, and ſome other obſerving men that deal in Optical Glaſſes, whether it had not been taken notice of, That there would ſometimes be, eſpecially in Winter and very moiſt Weather, a kind of Efflorescence of a ſaltiſh taſte manifeſtly diſcernable upon the ſurfaces of their Glaſſes; I was anſwered in the Affirmative, eſpecially by the above-mention'd Artificer, who having more occaſion and opportunity to take notice of ſuch things, told me that he had by taſting found theſe Exſudations ſenſibly ſaltiſh.

SECT. XIV.

And I was the more apt to entertain the lately propos'd Conjecture, becauſe of a thick Glaſs Cup that I have yet by me, in the making of which, to render it the more Diaphanous, I ſuppos'd an over great proportion of Salt had been employed. For this Cup though for a while it continued clear and entire, yet before the enſuing Winter was ended, though it did not ſo crack as to fall to pieces, but ſtill retains its former ſhape, yet it was flaw'd with ſuch a multitude of little cracks,

cracks, that at a diſtance it looks like a White, not like a Chriſtalline Cup.

SECT. XV.

I remember alſo that I have ſometimes, though not often, had Veſſels and other Bodies of Glaſs of a conſiderable thickneſs, which have of their own accord broken ſuddenly aſunder, with noiſe enough to make me take notice of it. And that this did not always happen for want of the Glaſs's being gradually or ſlowly cool'd, or, in the Workmens Language, neal'd, I was perſwaded not only by the ſpontaneous cracking, not without a loud noiſe of a thick and empty Glaſs Veſſel, that had for I know not how many Months been kept locked up in my Study; but by the like Accidents, which I after found had happened unto others. For enquiring of ſome that made great ſtore of Glaſs Veſſels, as well as of others that ſold them; I learned from both, that they had ſometimes by their loſſes been made to take notice that Glaſſes that had been long made, and kept unemployed would break of themſelves, when there was no viſible outward Agent near enough to be ſuſpected of the having broken them. And ſince this very Page began to be written, I had a fair Chriſtal Vial, not too well ſtopt, which crackt at the thick bottom, in a Glaſs-Cabinet (that was fixed to a Wall) where I kept that with other choice Vials under Lock and Key; no other of the included Glaſſes (full nor empty) nor yet of the external Glaſſes appearing any way crackt or injured. Nay even great and ſtrong Looking-Glaſſes are not quite exempted from theſe accidents. For I remember that having purpoſely enquired of an honeſt man that furniſhed the greateſt part of *London* with large Looking-Glaſſes,

Glaſſes, whether he did not ſometimes find them crack, and that with noiſe; he ſhow'd me divers large Plates of excellent Glaſs, and aſſured me, that ſometimes after they had been a good while in his Shop, ſome of them would of themſelves, not only crack with a loud noiſe, but now and then alſo (though rarely) fly aſunder with that violence as to break ſome of the neighbouring Plates though thick and ſtrong.

SECT. XVI.

And having alſo a mind to enquire further, whether this diſpoſition to break in ſome ſorts of Glaſs, might not continue much longer than I had opportunity to obſerve, I addreſſed my ſelf to an ingenious Maſter of a Glaſs-houſe, and demanded of him how long he had taken notice of Glaſs to continue ſound and whole, and yet afterwards to break of it ſelf. He replyed that he had once a great parcel of Glaſſes packed up, which not having the occaſion he expected to vend and make uſe of, lay by him for a great while; and yet when afterwards he had unpack'd them, and rang'd them, in a ſhort time a great many of them, amounting to about a fourth or third part of the whole number, cracked of themſelves; and when I asked how long the Parcels had lain by before they were opened, he replyed, that 'twas as he remembred between four and five years.

SECT. XVII.

Theſe Inſtances (to which I could adde divers others) I have therefore mention'd becauſe either of the two Hypotheſes in congruity whereunto they ſeem likelieſt to be intelligibly explicable, will favour the Doctrine hitherto patronized. For according to the Atomical Theory, it may be conceived that there is a conſtant inteſtine Motion of the ſmall parts of the Glaſs upon the ſcore of their conſtituent Atoms, which endeavour or tend to extricate themſelves and get away, which at laſt they do, by breaking the Glaſs in ſome brittle, or other fit place; where (after a multitude of encounters and evolutions) a competent number of them may happen to be got together, and find their Motion (outwards) withſtood: whence may enſue ſo unequal an agitation there, of the formerly coherent parts of the Glaſs, as to make the more agitated ones part from them that are leſs ſo; and conſequently crack the Glaſs. To which agrees what I have often obſerv'd in Chymical and Mechanical Tryals made with Glaſs-Veſſels, That if there be any grain of Sand or Gravel, or any little Lump of the Alkalizate matter Glaſs is made of, conſpicuouſly inclos'd in the ſubſtance of the Veſſel, 'twill both be much the more apt to break, and if it do, will almoſt always begin to crack at that place, (whence uſually as from a Center ſeveral cracks go ſeveral ways) the part of the Glaſs where the blemiſh is, being commonly of a differing Texture from the reſt (as is often manifeſt to the very eye) and being by that incongruous Texture diſpoſed to be put into a motion differing

differing from, and perhaps very diſproportionate to that of the neighbouring Parts.

SECT. XVIII.

I muſt not here ſtay to examine, whether or no this motion of the internal parts may not (in divers caſes be made more efficacious by the penetration of ſome ſubtle and moiſt matter into the Glaſſes Pores, (eſpecially the more ſuperficial ones of ſome Glaſs of a looſer ſort) and ſo by degrees vitiate the Texture of the Body, and promoting the Agitation and ſwelling of the ſaline Corpuſcles, enable them to burſt the Glaſs, after ſome ſuch manner as the Marchaſites I lately mentioned, came to have a vitriolate Efflorescence, and even to be burſt by the operation of the Air; this, I ſay, I muſt not now ſtay to examine, becauſe I would haſten to propoſe the ſecond Hypotheſis, and tell you that (elſe) we may, congruouſly to what we elſewhere diſcourſe, imagine, That in tract of time, there is produced in ſome parts of a Glaſs a Texture that makes it reſiſt more than it did formerly the free paſſage of the Æther, or ſome other ſubtle matter, that was accuſtomed (perhaps ſtream-like) to permeate it before; which tranſient matter now finding its paſſage obſtructed (and perchance almoſt quite hindered) by the vitiating of the Pores of the Glaſs, or ſome other (inconvenient) change of Texture in it, and endeavouring to continue its wonted motion through it, does ſo ſtretch the Pores, or otherwiſe offer violence to the Texture of the Body, that it cauſes a divulſion in

the parts, which according as it is more or leſs forcibly or ſuddenly made, does either barely crack the Glaſs, or make it flye aſunder. To the precedent Doctrine theſe two things agree not ill: The *Firſt*, That Glaſs is a Body eaſily made Electrical by rubbing, which makes it probable that its Particles may eaſily be put into motion. And the *Second*, That ſuch a Divulſion may be made in Glaſs by but an inequal motion between the neighbouring parts; as may appear by the Chymical practice of cracking Glaſſes, which they often think fit to do, only by applying a red hot Iron to the place till it be ſufficiently heated, and whilſt it is very hot, moiſtening it with cold water (or even Spittle) which by cooling the part that it touched, and conſequently checking the Agitation of the Corpuſcles it meets there, whilſt the contiguous parts retain their former vehement Agitation, occaſions a diſcontinuity or divulſion in the Glaſs, ſome of whoſe parts are in ſo ſwift, and others are in ſo ſlow a motion.

SECT. XIX.

And on this occaſion I ſhall adde chiefly, becauſe I would not pretermit ſo conſiderable a Phænomenon, That even when Glaſs ſeems to have loſt the degree of heat that one would think neceſſary to have its ſhape or bigneſs ſenſibly alter'd, there may remain yet ſo much agitation in the minute parts, as, when they are modify'd by the Air, by the Cold, or by ſome other inviſible Agent, to make them alter the bulk or ſhape of the whole Veſſel they compoſe; and that (which one would not expect) by enlarging

larging the Veſſel, at leaſt if we allow not in the caſe any change of Figure: For it has often been obſerved in thoſe Glaſs-houſes where they work White Glaſſes, as they call thoſe that are pure and clear, that when they have blown Glaſſes into a mold, to give them more exactly the deſired ſhape and ſize, theſe Glaſſes when they are cold cannot well be put again into the ſame mold they were blown in, and require that the Cells of Garde Vials that are to receive them be made a little larger: which Obſervation an eminent Artificer of my acquaintance that gets conſiderably by fitting fine Glaſſes to Caſes; has much confirm'd to me by his complaints of the inconvenience, eaſie to be incurr'd, by the not knowing, or not remembring ſo unlikely an Effect of the cooling of Glaſs. But I muſt not proſecute the conſideration of theſe, and the like Phænomena, nor examine which of the preceding Explications is preferrable, having mention'd them, as I was ſaying, chiefly to ſhew, that which of them ſoever we pitch upon, it will argue an inteſtine motion in the Corpuſcles of the Glaſs, which motion we ſhall think may be very ſlow, if we conſider how long a time it is on ſome Occaſions in producing its effects before it brings them to be diſcernable to our ſenſes.

SECT. XX.

Having thus made it probable, that among the parts of ſuch ſolid Bodies, as I have hitherto inſtanced in, there may not be ſuch a perfect Reſt, as is generally believed; it will I ſuppoſe be expected that I ſhould now draw this conſequence from what has been ſaid, That there is no ſuch thing as abſolute Reſt

Reſt in Nature. But ſince at my firſt mentioning this Paradox to you, I propoſed it but as Problematical, and ſince I conſider that we are not yet ſure, but that though many of the parts of ſolid Bodies may not be *always* moveleſs, yet ſome others of them may *ſometimes* for a while at leaſt be at perfect Reſt: I ſhall conclude as I began, and without reſolutely denying that there can be any ſuch thing *in rerum naturâ*, as abſolute Reſt, I ſhall content my ſelf to ſay, That 'tis not either abſurd to doubt whether there be or no; nor improbable to think that there is not, ſince we have not found it in thoſe very Bodies, where with the greateſt likelihood it might have been expected.

FINIS.